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WEISMANN AND HAECKEL: ONE HUNDRED YEARS[1]

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THE human spirit in which there exists the spark of desire to commune with itself on any philosophical question needs only the breath of a round-numbered anniversary to fan the spark into flame. When it is blown upon by two such anniversaries, even if or perhaps particularly if from different directions, there may well be a conflagration. And when the vagaries of some organized body's activities happen to present to such a spirit the opportunity of communing with others of like mind to furnish intellectual tinder, the result would naturally be a holocaust. I give you fair warning, but trust it may not be needed. The American Society of Naturalists has agreed to devote its energies to fundamental biological matters and specifically names evolution as an example. If, when the society conferred on me the privilege and duty of making this address, it had had its eye on the calendar of the centuries, it could scarcely have failed to foresee what topic would be selected.

What form shall our commemoration of the birth of the arch selectionist and the arch genealogist take? Shall we eulogize them? That has been done recently for both in public or semi-public ways. Shall we criticize them and their views? That was done roundly in their active lifetimes in a manner which must have been regarded by them as quite ample. Shall we bring forth their chief doctrines, dust them off to give them a deceptive freshness and proceed to find in them the germ of all the essential modern views of evolution? That is much the commonest way of celebrating anniversaries. Such commendation was years ago bestowed on Weismann by an

[1] Presidential address delivered at the annual dinner of the American Society of Naturalists at Pittsburgh, December 29, 1934. Contribution from the Zoological Laboratory of the University of Michigan.

apostle who saw in the then still adolescent Mendelian principles as complete a confirmation of his theory of the structure of the germ plasm as its author could ever have coveted. I do not now recall any similarly enthusiastic appraisal of the Gastraea theory, but I would myself be inclined to accord its foundation, the biogenetic law, a higher seat at the banquet table of biological doctrines than is now the fashion to assign it.

None of these programs bids fair to lead to a correct representation of present-day thought concerning evolution. Moreover, none of them befits my temperament. I shall therefore eschew eulogy and condemnation, and refrain even from exposition of the contributions of the celebrated duo of evolutionists whose names we are now pronouncing with whatever reverence we severally feel toward them. Far more important to all of us is the present status of that branch of biological science in which they were almost pioneers. It has taken strides recently, of what length I am sure we do not all now appreciate, and I propose to use the great centenarians merely as a yardage sign beside the evolution fairway to see how far we have come. To gauge this progress involves contemplation of the historical development of certain features of the general concept.

One of the most wide-spread characteristics of life is its adaptiveness. To many naturalists fitness is the one great attribute of organisms which needs and deserves explanation. In many quarters all else is secondary. Physiology and development are made to do anything, they are strained to the breaking point, and in justification of such distortion of their principles it is pointed out that only thereby can they be made to lead to utilitarian adjustment. Logic may be cracked if through the crevice thus created fitness to the environment may be revealed. Even the facts may be made to look different by gazing at them through glasses focused on the distant adaptation.

In the mind of the average biologist, theories of evolution are made or broken according as they explain or ignore, agree with or refute the common concept of adaptiveness. The strong appeal of inheritance of acquired characters, not only to Lamarck but to modern adherents of the idea, is the easy explanation which it is believed to provide for advantageous adjustment to surroundings. The success of Darwinism was no doubt due in large measure to its obvious explanation of fitness. If it worked at all, it should explain adaptation; and it is probable that there were many who, reluctant to accept evolution itself without a *raison d'être,* did so in view of the ready solution of the adaptation problem which natural selection offered. Darwin's most ardent followers were of the opinion that all characteristics of animals and plants are useful, and were persuaded that the problem of the evolution of any given quality was solved when the service performed by that quality was pointed out. Many of the extravagances of evolution theory of the latter third of the nineteenth century—extended, I fear, into the twentieth—are directly due to this belief in the ubiquity of adaptation.

To these utilitarians the discovery of so-called mutations by DeVries must have come as a distinct shock. At least, the interpretation put upon these genetic changes by DeVries must have seemed like a sweeping away of the very rocks at the foundation of evolution. It will be remembered that most of the changes in Oenothera which the great Dutch botanist discovered were not gene mutations, and should probably not now be called mutations at all. They were changes involving chromosome arrangement and, as would be expected, affected many parts of the plant. The designation "elementary species" applied to these extensive modifications by DeVries suggested a similar origin of species in nature. When smaller changes which *were* due to changes of genes were discovered, the name mutation was naturally applied to them, and the same general significance in evolution was attributed to them despite their smallness. Thus, in the opening years of the present century some of our most eminent evolutionists were of the opinion that species arose through mutation, without necessary aid from any other process, and that natural selection was no longer an essential guide. Curiously enough, Bateson, one of the chief early discoverers of presumptive mutations in nature and later one of the greatest manipulators of mutations in genetic experiments, never saw in these changes the building stones of evolution. To his dying day, apparently, Bateson was unable to picture the formation of species as occurring through the accumulation of mutations. His rejection of them, however, had nothing to do with their usefulness. Bateson set no great store by the adaptiveness of evolution. He was even one of the great critics of the school of selectionism which flourished, and still flourishes, in his own country. His dismissal of mutations as evolutionary changes rested upon their nearly universal fertility with their parent types. How, he asked, could species, which are usually intersterile, arise from a common stock through changed individuals which were at every step fertile with one another and with their parent types? The answer to his question we are perhaps in possession of now, but it must await its turn.

The experiments of Johannsen with selection in pure lines, and those of Jennings in clones were generally regarded as confirming the conclusion that natural selection is superfluous. Why they should have been so may now seem an oddity, since the experiments as a whole were not only in harmony with natural selection but actually proved the existence of

ixtures of genetic entities which is precisely the undation on which selection must work. But even ly three decades ago, to the average biologist varia- on was still variation. It is vain to point out, as is equently done, that even Charles Darwin knew that me variations are inherited, others not. Nothing is ore common than that a worshiper should seize upon me saying of his deity to prove that all knowledge, cluding that which is generally considered recent, as already his. A well-known entomologist, still ving, heatedly declared that Johannsen's genes were othing more than Darwin's gemmules, overlooking e two important facts that gemmules multiplied in e cells and were transported, neither of which ings the genes do. No, with all due honor to Dar- in, it can hardly now be maintained that he distin- uished two classes of variations. Certainly he did ot recognize a distinction based on fundamentally ifferent origins. It is possible, moreover, to see in Darwin's admission that variations are sometimes not inherited a belief that the very same variation occur- ing at another time might be transmitted. Much of his early amalgamation of variation into a single henomenon still existed in the early years of this entury. Under its influence, the failure of selection experiments to produce changes in the direction of election was regarded as another blow to the theory of natural selection.

Had evolutionists at that time looked away from he negative results of the selection experiments and ooked at the positive effects—had they turned from he failure of selection within pure lines and clones nd fixed their attention on the demonstration that nlike pure lines and clones exist together in popula- ions apparently homogeneous—they would have ensed more correctly the real significance of those experiments, and would have reversed their conclu- sions regarding the effectiveness of selection. A glimpse of what might have been deduced from the pure line and clone experiments was afforded by the experiments of Castle on hooded rats. The sorting, sifting action of selection was there fortunately seen at work upon the only kind of variation which can lead to evolution. Mendelism was, moreover, older by that time. Variation of a quantitative and appar- ently continuous type was soon thereafter seen to be as truly Mendelian as the simple sharply defined steps. Many intricacies of gene interrelationships were revealed, and the Mendelian system, which in the early years after the 1900 discovery was freely stig- matized as too simple to be true, was now growing too complex to be true.

It is ofttimes now regarded as strange that the new knowledge of genetics did not earlier influence evolu- tion theory. Every writer discussing the general field of evolution recognized that transformations of species involved heredity, and every such writer de- scribed the simple genetic processes and the mecha- nism on which they rest. But for many years not one of them attempted to show even approximately how evolution must proceed as a consequence of the operations of that mechanism. In explanation of this neglect it need hardly be pointed out that a mathe- matical mind and training are prime requisites to delving into the evolutionary processes dependent thereon. Fortunately, these essentials are now pro- vided in company with masterful attainments in the purely biological field, and at the hands of Wright, Fisher and Haldane we are gradually being shown what the course of evolution must be in the light of the Mendelian mechanism. We have learned what may happen to gene ratios without any particular cause; what should follow any definite schedule of mutation, particularly of repeated mutation; how migration affects the genic composition of a species; what consequences must flow from any selective ad- vantage or disadvantage of any gene or collection of genes; in what manner the expected results are modi- fied by the size of the population; and the very con- siderable differences in some of these influences de- pending on whether the population is growing, stationary or declining. It is already plain that there is much in evolution which most of us had not here- tofore suspected.

Let us pause to congratulate ourselves upon our accomplishments. I proposed a moment ago to use the great evolutionists born a century ago and actively championing evolution and its supposed factors half a century ago as distance signs to measure our prog- ress. Having taken our drive and located the evolu- tion ball approximately in its present lie, let us pace off the distance beyond our markers. We find Haeckel tracing pedigrees, and not greatly concerned with causes. Weismann, firmly convinced of the cor- rectness of the selection doctrine, was applying it to a wide range of animal characteristics. Indeed, it was to him universally applicable, since all qualities were apparently held useful. In furtherance of their individual fortunes, animals were held to have re- sorted to all sorts of ingenious devices—almost as ingenious as the theories of the evolutionists who were capable of detecting them. Their colors and shapes changed to render them inconspicuous to man, no questions being asked about their visibility to their real enemies. Ornaments arose in them in response to the love of beauty in one sex, when no other evi- dence of the esthetic sense existed. Strikingly gaudy patterns and blatantly obvious shapes sprang up to proclaim from the housetops with clarion silence the possession of a disagreeable taste or means of attack which ofttimes was not, from any other source than the warning, known to exist. Many a species was

held to have taken advantage of these danger signals to steal an unearned immunity from the predatory world by imitating those which gained a freedom from attack by direct methods. To this time also belongs the discovery of the defenseless animals whose mean individual psychological attainments entitled them to the once common designation "bromides," and which in keeping with these qualities invented the modern game of "follow the leader," and developed conspicuous marks on their hinder parts. To the spirit of that time, even if later in actual years, belong also the bogey colors, and we find it seriously proposed even in the latest edition of one of the great cyclopedias that a two-inch fulgorid bug owes its particular color to the advantage of resembling a crocodile. But I must end this recital of examples, lest you suspect I have inadvertently confused my dates by a few months and that it is really the centennial of Mark Twain which we are celebrating.

Truly we have come a long way. I fear, however, that the spirits of the preceding generation of evolutionists must be disappointed in us. We have progressed much more slowly than they evidently expected we would, and have taken a very different road. For Romanes, writing in the early nineties of the last century, shortly before his death, and advocating not only the Darwinian selection but the Darwinian inheritance of acquired characters as well, expressed the firm conviction that another ten years would see all outstanding difficulties in the way of evolution theory removed, and an era of good feeling inaugurated in which all evolutionists would be of like mind. This forecast reminds one of that other justly famous prediction of an artificial basic evolution, a wager laid at the turn of the century between an eminent physiological chemist and a celebrated cytologist, the stake being the best hat in New York City, that protoplasm would be created in the laboratory within ten years. Again we have failed in what was expected of us. It seems that in biology even Ten Year Plans have a habit of lagging behind schedule.

In view of the great expectations indulged in by biologists of a few decades ago, perhaps we owe ourselves a measure of justification. Is not our slowness really a consequence of our predecessors' speed? Have we not had to clear away endless corduroy, running crosswise and leading nowhere, before we could build the permanent pavement in a forward direction? Is there not even now a great deal of energy spent in hewing logs to repair the corduroy which might be spent in mixing concrete for the new highway? It would not be difficult to maintain that our present views of evolution would be sounder if there had been no direct study of it—certainly if there had been no speculation upon it—from the publication of "T Origin of Species" down to 1910 or even 1920. T is probably true of many great developments. T historical order is not the logical nor the economi one. The foundation facts on which a correct evo tion doctrine must rest would have been sought were sought—more for their bearing on physiolog genetics and embryonic development than for th relation to evolution. Hence the absence of evoluti speculation would have removed no stimulus nece sary to progress toward a correct solution of evo tion problems.

These comments are made, not with any belief th the rugged individualism of science by which any o was free to follow any whim and propose any theor could have been replaced by a new deal wherein on directed effort would be permitted, but only to expla our delay in arriving at sound results. It was inev table that easy speculation should have been preferr to experimental search for principles. Little el than speculation was at the foundation of the theori of warning color, mimicry and signal colors, for e ample. Supposed evidence for them of an observa tional sort was garnered from hither and yon with a the nonchalance of the smoker of a well-advertis brand of cigarettes. The colors of animals are n more marvelous if left unexplained than are th theories advanced to account for them. They are n more wonderful than is the ingenuity of man in ex plaining them. If we could explain the evolution o the human imagination we would have a better unde standing of the theories named than we can get now.

The danger from most theories is the heavy hand they lay on subsequent workers. Fisher, one of th leading exponents of the new approach to natura selection, refers to mimicry as "the greatest post-Darwinian application of natural selection." This remark is the reason for my perhaps unwarranted attention to these theories of color. Its truth or error probably hinges on the meaning of one word in it. The saving or damning ambiguity of the word "great-est" is that it may refer to magnitude measured in decibels rather than in pounds. I do not know the meaning attached to it by the author of the statement. I am reluctant to think that his valuation may be only an example of nationalism in science, though I can scarcely imagine it coming from a leader of an important modern biological movement in any intellectual country but one. I often wonder what scientific foibles we subscribe to in America because of a magnetic personality, powerful leadership or affable volubility. There must be some.

What does one do with a misapplied theory? Biologists have answered variously. The biogenetic law has been completely rejected in some quarters because the specific events which it was used to explain were

erroneously chosen. Even those who reject it admit the correctness of a modified form of the theory. One of Mendel's laws was found to be violated by linkage. But was it rejected *in toto*? No, it was modified to include all chromosome behavior. Natural selection, at the end of the last century, was judged—and still is judged by some—from the applications of it made by its supporters, and was by many able biologists rejected. The trouble with the natural selection theory is not that it will not work, but that it will not accomplish the results attributed to it. The germ theory of disease does not fall down when a mistake is made concerning the identity of the causative agent of a particular malady. It is even possible that the Nobel prize in medicine might be awarded for a discovery which proved later to be an error. What theory—what valid theory—is there that has escaped this fate of wrong application?

Merely to use a great principle to wrong ends is the simplest and least harmful of the untoward events that can happen to it. Such a principle might even be distorted to a considerable degree and still be worth rescuing, rehabilitating and recognizing by name. It is already evident that this is to be the destiny of natural selection. There are still some to whom natural selection means the explanation of a host of supposedly advantageous small characters which was the occupation of Darwinian supporters of a generation ago, some of whom still live. To these few, the word "selectionists" is still used to mean essentially the explainers of animal colors. It is a hopeful sign, however, that capable biologists have been able to approach the whole question of selection again in a semi-abstract form with no particular qualities in mind as needing, above all things else, an explanation. If architectural details can be forgotten for a time while sound plans dealing only with stresses and strains and strength and resistance of materials are evolved, it is likely that we may emerge with a practical and at the same time harmonious edifice. We may thus acquire a doctrine which, unlike the portmanteau theory derided by Yves Delage as capable of yielding up only what was put into it, will resemble more the magician's hat or the widow's cruse in dispensing far more than was first assembled in it—will indeed bring forth more than hopeful enthusiasm ever imagined it to contain. But this will not happen in ten years.

Having thus congratulated ourselves upon the magnificent distance of our drive, let us return to address the ball for the next stroke. Let it be borne in mind that our present position is not the result of a second shot starting from mimicry, sexual selection and similar concrete proposals. That ball was lost. Our recent drive started afresh from an alternative tee, namely, genetic phenomena. The club used is fashioned along fundamental Mendelian lines. We recognize that evolution consists of changes in the nature or arrangement of genetic units. These units are with few exceptions chromosomally contained, the known exceptions being certain plastid characters. A certain influence of cytoplasm upon early development has often been hailed as evidence that fundamental race characteristics are determined by the general protoplasm, but there are several indications that even this cytoplasmic influence comes under the control of genes within a generation. The genes in bisexual animals recombine in a fairly free way every generation. The restrictions imposed upon this recombination by inclusion of many genes in a single chromosome are rapidly removed by crossing over, so that in a long-time project like evolution they may be ignored. The genes may change, and change again, and return to any of their former states. Since evolution deals with populations, migration into and out of any group may change its composition. Relative proportions of the alternative genes in a collection of individuals determine the nature of the population, and these proportions are modified by the accidents of recombination, accidents of survival, changes in the genes and the accidental wanderings of individuals. If any gene or combination of genes confers an advantage that is expressed in increased relative numbers of descendants—and no other advantage is an advantage in evolution—that gene or combination gains over its alternates. A gene in one setting of accompanying genes and environment may have one effect, but in another setting a very different effect. The interplay of all these factors results in evolution so far as it depends on the regular mechanism of Mendelian heredity.

Using these fundamental features of the evolution mechanism Wright, Fisher and Haldane have formulated the expected consequences of each factor under various suppositions. They have postulated mutation rates, including reverse mutations, migration rates, selective advantages and various population sizes, and out of them have pictured the evolution process in the abstract. Along with this service they have performed two others of which they may not have been aware. Their mathematical treatment has served to take evolutionists out of themselves, to make them less introspective, to force them to look for factors of evolution outside of their own minds, which is where most of them have been looking heretofore. It has also helped to develop an inferiority complex where one was badly needed but never before existed. While their mathematical discussions are frequently summarized in plain language, there are many parts of them which impress a non-mathematical person with his own incapacities. They show that many means of acquiring a valid opinion are closed to such a person.

How immensely valuable it would have been to the whole structure of evolution theory to have had such treatises circulated among naturalists just when the theories of animal coloration, for example, were being promulgated! How our friends the economists and psychologists must envy us now the possession of one small means of reducing the number of those who feel qualified to speak!

The debt we owe to our mathematical-minded friends is obvious. Any one who acknowledges that two and two make four should recognize this obligation, even if he does not follow their reasoning. So great is our debt to them that it would be ungrateful not to point out any shortcomings we think we see in the purely biological assumptions they make. It was Johannsen, I believe, who adjured us to treat our biology not *as* mathematics, but *with* mathematics. None of these leaders is under the illusion that statistical methods alone will solve all evolution problems, but it is easy to argue from mistaken premises. This I think at least one of them has done.

An error of a purely biological sort I deem to have been made by Fisher relative to the direction of mutation. If mutation is the beginning of every evolutionary change, obviously evolution can not proceed in a direction in which no gene changes. Fisher plainly assumes that mutation is purely random with respect to direction. When he merely states that mutation is random, it would be possible to suppose that he means fortuitous as to time or locus. This can not be all that he does mean, however, for his philosophy of evolution requires in several respects that mutation be random as to quality as well. Ford, who has been associated with Fisher in some evolutionary projects, and who apparently takes his statistical cue from the latter, is more specific, and claims explicitly that mutation is purely fortuitous in quality (that is, in the nature or direction of the changes) as well as in locus. It would be easy to imagine that support for this assumption has been given by competent geneticists when really they offer no such corroboration. Muller, for example, points out that mutation is random, but means thereby only that it is non-adaptive, that its nature is not determined by the environment. Fisher's assumption clearly is that mutation is happening, not just in every *possible* way (the possibilities being limited by the structure of the genes), but in every *conceivable* way. He holds that mutation provides *every* avenue of progress and that something chooses among the radiating paths. That thing he holds, as do we all with less sweeping premises, to be natural selection. Whether his belief in the randomness of direction of mutation has led him to his faith in the Allmacht of selection or whether his conviction that selection is all-powerful moved him to conclude that mutation must be random in quality is uncertain. The two are closely bound, and their adoption by Fisher constitutes, in my opinion, one of the weaknesses of his position.

There are many reasons to conclude that mutation is not random in quality, but is directed. This does not mean that a mutation occurring at a particular time and place must be of only one sort, though it is conceivable that even this might be true. Still less does it mean that successive mutations at the same locus are likely to go farther and farther in the same direction, though even this has been held probable by at least one geneticist. It means merely that some of the conceivable paths, probably most of them, indeed, are closed. The repeated occurrence of certain mutations, like that to white eye in Drosophila, is an indication of such guidance. So is the production of parallel mutations in related species, as the occurrence of ruby eye and a number of other mutations in two species of Drosophila or a number of color mutations in mice, rats, rabbits and guinea pigs. It strains one's faith in the laws of chance to imagine that identical changes should crop out again and again if the possibilities are endless and the probabilities equal. Reverse mutations would be very unlikely if the direction of mutation were purely random. Were eye color mutations random in quality, every color in the rainbow should be represented. The fact that in Drosophila many shades of red have arisen, while not once has there been a mutation to green or blue, leads to a suspicion that for some reason these latter colors are impossible. All this limitation is rendered *a priori* very probable by the fact that genes must be chemical entities, and that no chemical substance is capable of reacting in every conceivable way. To assert, as Fisher does, that mutation has nothing to do with the direction of evolution is like assuming that a tetrahedron may fall, at different times, with ten or a hundred points uppermost. The ten points and ten opposite sides to fall upon do not exist. How great a restriction is placed upon the course of evolution by the inability of genes to mutate in certain ways it is impossible to tell; but it may easily be much greater than any of us suppose.

A logical consequence of the belief that mutation is wholly random in quality is the conclusion that species change promptly and perhaps rapidly in that direction in which their own best interests lie. If mutations really do occur in every conceivable direction with equal probability, and some of them confer advantages, there can be no reason why a species should not start at once in one or more of the favorable directions, at a speed dependent on the frequency of mutations in those directions and the degree of advantage afforded by them. Thus, a species should at any moment be about perfectly adapted to its environment. And, indeed, we are told that this is what

we should expect. Alas! When I reflect that, under a physician's orders, I must forever refrain from the delectabilities of apple pie, I am reluctant to believe that organisms are as well adapted as they might conceivably be. And if I am reminded that the defect which I have cited is that of an individual, not of a species, I need only add that if I were making a human race perfectly adapted to its environment I should certainly wish to endow it with an enzyme that would digest cellulose. It seems clear to me that species are not, probably in any instance, as well adapted to their environments as they could be. They may be approaching as close an adaptation as is permitted by the mutations arising in them, but that must fall far short of perfection. For every living kind this best of all possible worlds must yet have room for improvement.

Fisher has adopted views of the origin of and reason for dominance with which Wright and Haldane are unable to agree. In so far as his opinion rests on the assumption that genes produce different effects, depending on the company they keep, it is well enough grounded. But the smooth working of the scheme of growing dominance demands again a steady flow of random—qualitatively random—mutations. The mere fact that wild type genes are so generally dominant over their mutants indicates that, if Fisher's theory of becoming dominant through accumulation of accessory genes which increase dominance be correct, this accumulation must occur relatively rapidly. That is, genes of the right kind must always be quick to appear, no matter what is demanded of them. This could only happen if genes of every conceivable kind were appearing with what must be considered, in evolution, great frequency and regularity.

This concept of gradually changing dominance is probably responsible, at least in part, also for Fisher's sliding scale of gene effects. He states that if a change of 1 mm in some quality has selective value, a change of .1 mm in the same quality and in the same direction will have approximately one tenth as great selective value. When I read this statement I recall that I, as a boy at the county fair, strove with mighty blows to lift the heavy weight until it rang the bell, but never succeeded. I could get three fourths or even nine tenths of the way. Had I held Fisher's philosophy, I should have demanded three fourths of the Negro doll which was the reward of success; but I would have been denied. In living things, just as in carnivals, there are thresholds which must be reached before any effect is produced. The new philosophy of natural selection has not abolished them. They exist in development, in general physiological processes, and I doubt not in adaptation.

We have here a group of more or less related situations in which a wrong reading of biological facts may easily lead to wrong conclusions, despite the most careful of mathematical calculations. I yield to no one in my satisfaction in the progress recently made in attacking evolution over the mathematical route. But we should be exceedingly careful to base the calculations upon sound biology.

At still another point on the new battle front is there need of consolidating our positions. In the late war, when an official bulletin described the activities of the military as a consolidation of its gains, it meant that the army had retreated. Perhaps that is what is needed in the present evolution skirmish, at least in certain places. The consolidation is needed in our attitude toward the origin of those qualities which have no value. I know that I shall be challenged concerning what I am about to say, but I do not entertain any doubt whatever that living things are possessed of characteristics that are of not the slightest use. In an ordinary assembly I might defend this position by illustrating it only from mankind, and asking my hearers merely to look about them for examples. Before the American Society of Naturalists, however, it seems necessary to go afield and refer to species in general.

Most systematic workers appear to be convinced, at least with respect to the groups with which they are familiar, that the differences between nearly allied species are chiefly useless distinctions. Students of animal color, particularly of insect color, should take notice. They are prone to claim a use for specific distinctions in color; in view of the apparent non-adaptiveness of other species differences, however, it is far safer to acknowledge usefulness of color only after the most complete proof of it has been obtained.

It is these useless characters which constitute one of the puzzles of evolution. They are a particularly heavy load upon the natural selection doctrine. Nor are specific characters the only ones that fall in the seemingly non-adaptive class. Many great evolutionary developments in the past give every indication of having been without advantage, such as the curious armatures of some of the huge mesozoic reptiles and some of the extinct mammals. So numerous are the apparently useless characters of organisms and the ostensibly non-adaptive changes in them that most evolutionists have felt obliged, at one time or another, to postulate some other factor besides natural selection to account for them. Could such a factor be discovered and be proved general, most of the outstanding problems of evolution would be started on the way to solution. What the world most needs, then, is not a good five-cent cigar, but a workable—and correct—theory of orthogenesis.

Unfortunately the Lamarckian principle that the

need of or desire for a thing helped to bring about its development does not apply to biological theories any more than to animal characteristics, and no acceptable theory to account for wide-spread non-adaptive evolution has ever been devised. Of the leaders of the statistical movement in the study of evolution, only Fisher seems oblivious to the need of such a principle, perhaps because he recognizes only a very small minimum of useless qualities. Wright is fully cognizant of the abundance of non-adaptive characters, and allows accident to explain the smaller examples, up to the level of varietal or even specific differences. He is aware, too, that much more than this is needed. Haldane is conscious of his possible ignorance in this small field of evolution inquiry, namely, the adaptiveness or non-adaptiveness of specific differences, but has made a brief but serious attempt to give an explanation to orthogenesis. He suggests that the genes producing the useless adult character may have a selective advantage in the embryo. Certainly no general theory of evolution can go unchallenged which does not permit the origin of unadaptive characters. I wonder whether the mathematical possibilities of non-adaptive evolution have been as thoroughly explored as they may be. I trust that the ray of hope which formulas, curves and equations have given us has not emitted its last flicker in this particular direction.

While waiting for possible further developments in the statistical field we need not stand idle. Some of the most significant of evolutionary changes appear at present not to be amenable to mathematical study. Their exemption from statistical formulation derives first from their infrequent occurrence (and statistics is ever based on considerable numbers, so that random opposite effects may tend to cancel one another) and second from the uncertainty whether they are wholly accidental. To shorten the discussion I shall pass by, despite their importance, the very quick transitions from one species to another, not through gene mutation and shifting of gene ratios, but by polyploidy, discovered so much more abundantly in plants than in animals. I shall likewise omit consideration of interspecific hybridization as a means of attaining recombination of genes.

It is rather to the isolation of species from one another that I shall turn for what seems to me some of the most significant of recent advances. For isolation has been in the past as far from a satisfactory explanation as any other major phase of evolution, excepting only non-adaptive modification. Though geographic isolation was first in the minds of evolutionists who realized the importance of insularity of groups in the development of differences between them, it has played little part in the new forward strides toward a knowledge of the segregation of species. It seems now a little odd that geographic separation ever should have been relied on to produce so many of the differences that were observed to exist between closely allied species. True, it aided each group in experiencing a different series of shifts of gene ratios, and could easily have brought about the visible distinctions between species. What it could not do, in accord with anything then known of heredity, was to produce the sterility, partial or complete, which nearly always arose between such groups along with their visible differences. This sterility was not lightly to be dismissed. So characteristic of species is their sterility with other species that the late Professor Bateson was ready, as already pointed out, to reject mutations as the building stones of evolution because every known mutant could be crossed with its parent type with resultant fertile progeny. How could intersterile species arise out of interfertile mutations? It was of course conceivable that differing aggregations of genes would entail a reduction of fertility if brought together. Something is known to-day of combinations of genes that induce sterility between types, but these are genes whose only known effect is that upon fertility, and it is combinations of these particular genes, not combinations of general gene complexes, which cause incompatibility of gametes or infertility of zygotes. But even if there had been some plausibility in the assumption that accumulation of gene differences of any sorts whatever would gradually lead to intersterility, evolutionists of the period to which we are looking back should have been cautious in attributing sterility to geographic isolation, for they found the same sterility to exist between types for which there was no apparent reason for geographic separation. They frequently took refuge in the belief that geographic isolation existed where none could be seen; but imagining barriers where none was apparent was as objectionable a procedure as hypothecating anything else merely to save a theory.

Although a master key to the problem of interspecific sterility has not been found, at least one individual solution and several clues have been discovered, none of which bear any relation to environment. Geographic barriers may exist, but they are not necessary to the separation of taxonomic entities. And the search is rightly being made among genetic phenomena and their related cytological events.

Only a little while ago there was a strong hope that abundant sources of intersterility would be found in the phenomena of meiosis, knowledge of which is itself but little more than a generation old. Organisms have solved the problem of approximate stability of type, coupled with a degree of genetic plasticity, by adopting, along with gamic reproduction, the synapsis of homologous chromosomes fol-

lowed by their regular separation in the formation of the gametes. This method insures the viability of by far the majority of all combinations, in place of the chaos and excessive mortality which must occur in the absence of any such scheme. Yet it provides slow change through random recombination of chromosomes as groups, and fairly rapid reconstitution of individual chromosomes through crossing over.

Into this mechanism there creep occasionally such changes as inversions, duplications and translocations—rearrangements of genetic material without any necessary changes in its units. These irregularities were eagerly studied in the search for causes of intersterility. For, since the rapprochement of homologous chromosomes in each generation is apparently due in large measure to the similarity of the genes they contain and of the arrangement of those genes in the chromosome, the occasional changes in this arrangement just referred to might reasonably be expected to prevent much of the usual synapsis, with consequent irregularities of meiosis, and deficiencies or surpluses of genes in the several gametes. Some loss of fertility would naturally follow each such irregular event. It was conceivable, however, that among the products of the irregularity two similarly disarranged chromosomes might have the good fortune to meet in fertilization, along with the usual (or some other viable) combination of the remaining chromosomes, and a new type to be the result. Such a type might differ little or not at all from the original, yet it seemed plausible that there should be a degree of infertility of their hybrids because of the unmatched constitution of their chromosomes.

I speak of these discoveries in the past tense and subjunctive mood because there is now considerable doubt of the efficacy of the disarrangements of chromosome parts. The only well-known species in nature differing chiefly by an inversion are two species of Drosophila, and though their hybrid is sterile as expected, that sterility is evidenced by degeneracy of the reproductive system at a time too early for synapsis. Likewise, the best known translocations have not led to sterility of hybrids. The outlook for an explanation of intersterility as a consequence of chromosome aberrations is thus somewhat dimmed, though it can hardly be said that the possibilities have been explored and the method should not be abandoned as useless until more instances of its inadequacy are known.

No great despondency need descend upon us, however, because of the failure of such obviously possible explanations of intersterility to meet our early expectations. We have left to us the sterility genes which have been most carefully studied in various relations within species. All that is needed is to extend these relations to crosses between species, and to postulate dominant complementary genes, each existing harmlessly in one species but blocking some essential reproductive process when together with another in the same individual, as in the species hybrid.

Regardless of the nature of the cause of sterility of hybrids, once such a mechanism is in existence in two individuals or groups of individuals, all that is necessary thereafter is that different mutations shall occur in the two types—mutations which can not be transferred from one to the other by hybridization—and two species are distinguishable. These things are well known, I think, to all of you; but their significance has not yet crept into the consciousness of the evolution fraternity in general. Their possibilities with reference to the isolation of species are endless. Moreover, so spontaneous may they be that sterility between types may spring up anywhere. No longer do we need to postulate a considerable divergence of types before intersterility arises. Sterility may originate early in the process of separation, or even before any other modification commences. It may be and probably is one of the primary reasons for the splitting of species, since any changes that do arise by mutation or otherwise are thereby removed from some of the leveling influence of hybridization. If this surmise is correct there may be hosts of incipient species about us, differing in no observable respect from other members of what is still called their species, but possessing already the quality which renders them incapable of breeding with certain of their fellows. Whether these partially isolated groups survive the accidents of elimination, the chances of breeding with those with which they still are fertile and other factors of preservation are other questions which may now be regarded as secondary.

We have arrived at our destination, namely, the present-day concept of evolution. At least we have approached as near that goal as the guide is licensed to conduct parties. We have traveled laboriously, and naturally look back upon the distance traversed with satisfaction. We may assume a supercilious attitude toward our predecessors, and view their evolutionary ideas with scorn. Their theories may seem to us conceived in romanticism, and their arguments to be a cobweb of irrationality. In the words of a popular writer, on whom I tried out the general drift of this address before bringing it to you, we may regard their period as one of bewilderingly obfuscatious scientific hallucination, abbreviated in this day of governmental alphabetics to BOSH. But even though this judgment were correct it would little behoove us to harbor it. Far more important than to congratulate ourselves upon our accomplishments is to acknowledge how much is still undone. While I am quite unwilling to share with Osborn, Barbour and some others the view that we are still as ignorant of

the factors of evolution as biologists were a generation ago—a few biologists may still be—I am quite certain that even a moderately full knowledge of them is still far beyond. Even were the only outstanding difficulty the existence of non-adaptive qualities in organisms we should be still far from finality; but this I regard as the heaviest task before us. The alternative of this task, which has sometimes been proposed, namely, a denial that any evolution is non-adaptive, is not to be considered until every other possibility has been thoroughly explored. For most of us a time which is ripe for such denial will never come, for the necessary explorations of other leads can not possibly be made in many times ten years.

SCIENTIFIC EVENTS

THE TWENTY-FIFTH ANNIVERSARY OF THE BROOKLYN BOTANIC GARDEN

INVITATIONS and announcements have been issued for the celebration of the twenty-fifth anniversary of the Brooklyn Botanic Garden from Monday to Thursday, May 13 to 16. The programs fall under four headings—civic, social, scientific and educational.

On Monday evening the president of the board of trustees, Edward C. Blum, will preside. The speakers include the president of the borough of Brooklyn, the Honorable Raymond V. Ingersoll; the commissioner of parks, the Honorable Robert Moses; the president of the board of education, the Honorable George J. Ryan, and the chairman of the Botanic Garden governing committee, Miss Hilda Loines. The principal address will be given by Dr. Albert F. Woods, director of the Graduate School, U. S. Department of Agriculture. The program will be followed by a reception and inspection of exhibits illustrating the progress of development of the Botanic Garden since 1910. A feature of this exhibit of special scientific interest will be a selection of some of the incunabula and other rare books and manuscripts in the Botanic Garden library.

On Tuesday afternoon the twenty-first annual spring inspection of the garden, with the Honorable Fiorello H. La Guardia, mayor of New York, as guest of honor, will be held. This will be in charge of the woman's auxiliary of the garden.

The scientific programs deal with the progress of various aspects of botanical science during the past twenty-five years, as follows:

Wednesday Morning: Presiding, Professor R. A. Harper, Columbia University.

(1) "Virus Diseases of Plants: Twenty-five Years of Progress, 1910–1935." Dr. L. O. Kunkel, Rockefeller Institute.

(2) "Twenty-five Years of Cytology, 1910–1935." Professor Charles E. Allen, University of Wisconsin.

(3) "Twenty-five Years of Genetics, 1910–1935." Dr. Albert F. Blakeslee, Carnegie Institution of Washington.

Wednesday Afternoon: Presiding, Professor Edmund W. Sinnott, Barnard College.

(1) "Twenty-five Years of Plant Physiology, 1910–1935." Professor Rodney H. True, University of Pennsylvania.

(2) "Light on Vegetation, 1910–1935." Dr. John M. Arthur, Boyce Thompson Institute for Plant Research.

(3) "Twenty-five Years of Ecology, 1910–1935." Dr. H. A. Gleason, New York Botanical Garden.

(4) "Twenty-five Years of Forestry, 1910–1935." Dean Samuel N. Spring, New York State College of Forestry, Syracuse University.

Wednesday Evening: Presiding, Dr. William Crocker, Boyce Thompson Institute for Plant Research.

(1) "Twenty-five Years of Plant Pathology, 1910–1935." Professor L. R. Jones, University of Wisconsin.

(2) "Twenty-five Years of Systematic Botany, 1910–1935." Dr. Elmer D. Merrill, New York Botanical Garden.

(3) "Twenty-five Years of Paleobotany, 1910–1935." Dr. G. R. Wieland, Carnegie Institution of Washington.

(4) Motion picture (silent)—"The Life Cycle of a Fern." Harvard film. Premier showing.

Thursday morning will be devoted to a horticultural program, with John C. Wister, director of the Arthur Hoyt Scott Horticultural Foundation, Philadelphia, presiding. The papers are as follows:

(1) "Twenty-five Years of Horticultural Progress, with Special Reference to Foreign Plant Introduction, 1910–1935." Dr. W. E. Whitehouse, U. S. Department of Agriculture.

(2) "Opportunities for Women in Horticulture, 1910–1935." Dr. Kate Barratt, the Swanley (England) Horticultural College.

(3) "Growing Plants in Sand with the Aid of Nutrient Solutions: With Special Reference to Practical Applications." Professor C. H. Connors, Rutgers University.

(4) "Modern Methods of Plant Propagation." Dr. P. W. Zimmerman, Boyce Thompson Institute for Plant Research.

(5) "Plant Patents." Colonel Robert Starr Allyn, deputy commissioner of sanitation, New York City.

(6) Motion picture—"Naturalized Plant Immigrants." U. S. Department of Agriculture, Bureau of Plant Industry.

The Thursday afternoon program will be given to

botanical education, including elementary education. The papers are as follows:

Presiding: Dr. John S. Roberts, associate superintendent of schools, New York City.

(1) "Botanical Education for Young People." Dr. D. W. O'Brien, the School Committee of the City of Boston.

(2) "Twenty-five Years of Botanical Education, 1910–1935." Professor Otis W. Caldwell, Columbia University.

(3) Motion picture—"How Seeds Germinate." U. S. Department of Agriculture, Bureau of Plant Industry.

The first program on Thursday evening will be devoted to adult education in botany and the newer techniques of education developed since the Botanic Garden was established.

Presiding: Julius M. Johnson, president, the New York Association of Biology Teachers.

(1) "Adult Education in Botany." Dr. Loren C. Petry, Cornell University.

(2) "Radio in Botanical Education." Morse Salisbury, U. S. Department of Agriculture.

(3) Motion pictures—"Their Part in American Education." Dr. Clarence E. Partch, Rutgers University.

(4) Demonstration of silent "movies" and "talkies": (a) "Time-Lapse Studies in Plant Growth"—1 reel, U. S. Department of Agriculture film; (b) "Plant Life" (a sound film)—1 reel, Harvard film service.

There will be an invitation buffet luncheon on both Wednesday and Thursday, a tea on Thursday afternoon in charge of the Junior League of Brooklyn and informal receptions with inspection of the exhibits on both Wednesday and Thursday evenings. The hostess on Thursday evening will be the Garden Teachers Association of the Botanic Garden. Members of the Boys and Girls Club of the Botanic Garden will also assist throughout the week.

On all days there will be opportunity to inspect the plantations and collections under guidance. All persons interested in botanical science and education are invited to attend the Wednesday and Thursday programs.

EXPEDITIONS OF THE SMITHSONIAN INSTITUTION

THE results of twenty expeditions sent out last year by the Smithsonian Institution are described in a report recently issued. Collections of biological, geological and anthropological specimens for the U. S. National Museum were made in China, Siam, Mexico, South America and the Galapagos Islands, as well as in the United States and its territories.

Dr. Charles G. Abbot, secretary of the institution, and L. B. Aldrich, of the Astrophysical Observatory, conducted an expedition of two and a half months' duration to Mount Wilson, Calif., where extensive astrophysical work was conducted. Throughout the year daily observations of solar radiation were made at the stations at Table Mountain, Calif., Mount Montezuma, Chile, and Mount St. Katherine in the Sinai Peninsula.

The Rev. David C. Graham, in the high mountain regions of the Szechwan Province of China, made natural history collections, obtaining such rare animals as the golden-haired monkey, the giant panda, the blue sheep, the horse-tailed deer and the Chinese red wolf. Dr. Hugh M. Smith, associate of the institution, formerly fisheries adviser to the Siamese Government, penetrated the wild country at the head of the Pasak River. He made there collections of rare birds and several new species of fishes. Dr. W. F. Foshag hunted rare minerals in the Sierra Madre Mountains of Chihuahua, Mexico, and in other Mexican mining districts. Dr. C. Lewis Gazin sought the bones of extinct animals in the Snake River basin of Idaho and in one place obtained the skulls of about sixty-five ancient horses. Studies of ancient fauna in southwestern Ontario and in Michigan were carried on by Dr. G. A. Cooper, assistant curator of paleontology, who was associated with Dr. A. S. Warthin, of Vassar College. Their researches enabled them to construct a partial, tentative picture of the country during the Devonian period.

Dr. Waldo L. Schmitt, curator of marine invertebrates, represented the institution on the Galapagos Island expedition of Captain G. Allan Hancock. An exceptionally rich natural history collection was obtained, including ten species of poisonous sea snakes. A study of the butterflies of Virginia was undertaken by Austin H. Clark, curator of echinoderms. About 8,000 specimens of grass, including some rare species, were obtained by Jason R. Swallen in the mountains of Brazil. C. W. Bishop, assistant curator of the Freer Gallery of Art, carried out an archeological reconnaissance in China over an area approximately 500 miles in length by nearly 200 in breadth. Dr. Aleš Hrdlička, curator of physical anthropology, continued his excavations on Kodiak Island, Alaska, where he found evidence of a great prehistoric massacre.

Archeological projects were conducted in the valley of the lower Columbia River by Herbert W. Krieger, curator of ethnology; in Florida by CWA workers under the direction of Matthew W. Stirling, chief of the Bureau of American Ethnology; on the Shiloh battlefield by Frank H. H. Roberts, Jr., and in California by William D. Strong and Winslow M. Walker. Dr. John R. Swanton reports progress in tracing the

route of De Soto through the southeast, Dr. John P. Harrington continued his researches among the Indians of California and Dr. Truman Michelson studied the Passamaquoddy Indians of Maine.

THE EXPOSITION OF CHEMICAL INDUSTRIES

THE fifteenth Exposition of Chemical Industries will be held at Grand Central Palace, New York, from December 2 to 7. It is said that it will be one of the largest in recent years, and that the volume of requests for space makes necessary the early preparations which are being made for this year's show. Some of the more pretentious exhibits are being designed and constructed over a period of eight to ten months in advance of the exposition week and companies leasing the smaller exhibition spaces are making their contracts many months in advance. At the last exposition, held in 1933, the attendance was from 983 cities and towns in 42 states of the United States and from 69 cities and towns in 27 foreign countries. The registered attendance was 34,269, representing an increase of 50 per cent. over the previous exposition. Admission is without charge and by registration or invitation only. No tickets are sold.

The Exposition Advisory Committee will include distinguished representatives from all the leading chemical organizations.

Members of the Advisory Committee are as follows: A. D. Little, Arthur D. Little, Inc., *chairman;* Raymond F. Bacon, consulting engineer; L. H. Baekeland, honorary professor, chemical engineeering, Columbia University; Wm. B. Bell, president, Manufacturing Chemists Association; J. V. N. Dorr, president, the Dorr Company; A. E. Marshall, president, American Institute of Chemical Engineers; Henry B. Faber, consulting chemist; John M. Alverez, president, Salesmen's Association of the American Chemical Society; Williams Haynes, president of Chemical Industries; Charles H. Herty, industrial consultant; H. E. Howe, editor, *Industrial and Engineering Chemistry;* James H. Critchett, president of the Electrochemical Society; Sidney D. Kirkpatrick, editor, *Chemical and Metallurgical Engineering;* Roger Adams, president of the American Chemical Society; L. H. Marks, president of the Chemists' Club; W. T. Read, Rutgers University; H. J. Schnell, general manager, *Oil, Paint and Drug Reporter;* T. B. Wagner, consulting chemist; R. Gordon Walker, vice-president, Oliver United Filters, Inc.; M. C. Whitaker, consulting chemist, and Fred W. Payne and Charles F. Roth, co-managers of the exposition.

AN EXHIBIT OF RARE PREHISTORY MATERIALS

THE Department of Anthropology, University of Minnesota, is fortunate in possessing an unusual number of unique prehistoric human skeletal and artifact materials which are now being placed on exhibition for the first time. They will be available in Wesbrook Hall, Main Campus, for the meeting of the American Association for the Advancement of Science, which will be held from June 24 to 29.

Among the most important specimens are the following:

"Minnesota Man," type skeleton of oldest known accredited man in Western Hemisphere, and two artifacts found therewith;

"Mechte-el-Arbi," type-skull of North African Capsian or Getulian culture, of some 30,000 years ago. Besides, there is the extensive Debruge archeological collection from North Africa;

Twelve additional Mechte-el-Arbi skulls—four of which were dug by the University of Minnesota, together with 6,000 flints of African shell-heap culture, some 30,000 years old, also dug by the University of Minnesota;

One half of the type-specimen flint artifacts of Capsian or Getulian African culture;

The type-specimens of Mousterian stemmed "points" from Africa;

The first Mousterian coup-de-poing from the type-site at Le Moustier, France;

The type artifacts of the ivory culture of mid-North America;

Two atlatl stone-weights found in Minnesota;

Typical Yuma flints found in Minnesota;

Typical Folsom flints found in Minnesota;

Four, perhaps unique copper fishing gorges from a Minnesota habitation site;

Minnesota "Browns Valley Man," type-skeleton and six Yuma-Folsom flints found with said type skeleton;

Extremely rare, if not unique, evidences of abundant cannibalism among one group of mound-burying Minnesota Indians;

Extremely rare Minnesota pottery from both habitation sites and mound burials.

ALBERT ERNEST JENKS

THE LIBERTY HYDE BAILEY HORTORIUM

ONE of the largest private herbariums in the country, including comprehensive records of the cultivated plants of the world, has become the property of Cornell University as a gift of Dr. Liberty Hyde Bailey, professor emeritus of agriculture, and Mrs. Bailey. The collection, which will be designated as the Liberty Hyde Bailey Hortorium, comprises upwards of 125,000 mounted herbarium sheets and other similar material, especially rich in the cultivated floras of the world and comprising types of new species in the palms, Carex, Vitis, Rubus and other groups; there are included 4,000 technical and professional books and thousands of photographs and card indices with working equipment. The buildings which house these collections and about a quarter of an acre of surrounding land are included in the gift. For the past fifteen years illus-

trated publications have been issued from the establishment under the general title Gentes Herbarum.

In offering the collection to the university, Dr. Bailey wrote:

> The value of these collections depends on the use that is made of them. The accumulations have been assembled over many years with the hope that they may constitute the basis of a departure in education and research, a new unit unlike any now in existence and which need not duplicate the field of any other department. Its primary purpose is to record and study the cultivated flora of the world to the end that the species may be accurately identified as a scientific basis in horticulture, plant-breeding, pathology and any other departments of knowledge that work with domesticated plants; and to provide archives of the plants that men at any time or place may grow.

The university has authorized the establishment of an administrative unit in the College of Agriculture to be known as the Liberty Hyde Bailey Hortorium, to be placed under the direct supervision of a staff member. A full-time curator, whose duty shall be the general care and supervision of the hortorium, will also be appointed. There will be an advisory board consisting of representatives of the major fields of plant science and two members at large appointed by the president, together with the supervisor of the hortorium and the curator. One or more graduate fellowships, to be known as the Liberty Hyde Bailey Botanical Fellowships, will be established.

RECENT DEATHS

DR. CHARLES E. ST. JOHN, research associate at the Mount Wilson Observatory of the Carnegie Institution of Washington, died on April 26. He was seventy-eight years old.

DR. ERNST BISCHOFF, head of the chemical and pharmaceutical firm of the Ernst Bischoff Company, Inc., New York, died on April 19, at the age of seventy-one years.

THE death is announced of Dr. J. Loring Arnold, professor emeritus of electrical engineering at New York University. He was sixty-seven years old.

RICHARD MORRIS HOLMAN, associate professor of botany at the University of California, died suddenly on April 23, aged forty-nine years.

THE death is announced of Sir Richard Rawden Stawell, of Melbourne, Australia, president-elect of the British Medical Association.

HERBERT BRERETON BAKER, professor emeritus of chemistry at the Imperial College of Science, London, died on April 29, aged seventy-three years.

SCIENTIFIC NOTES AND NEWS

DR. HERBERT SPENCER JENNINGS, professor of zoology at the Johns Hopkins University, has been elected Eastman professor at the University of Oxford for the academic year 1935–36. Dr. Arthur H. Compton, professor of physics at the University of Chicago, is this year Eastman professor. The professorship was founded by the late George Eastman to send American scholars to Oxford, and the endowment is in the hands of the Association of American Rhodes Scholars.

ON the occasion of the annual dinner of the National Institute of Social Science the gold medal of the institute was awarded to Dr. Harvey Cushing, since 1933 Sterling professor of neurology at Yale University.

AT the annual convocation of the American College of Physicians, which met in New York City during the week of May 1, presentation of the John Phillips Memorial Medal was made to Dr. Leo Loeb, professor of pathology at Washington University, St. Louis. Professor Loeb delivered the convocation oration, speaking on "The Thyroid-stimulating Hormone of the Anterior Pituitary Gland." Dr. O. T. Avery, of the Rockefeller Foundation, to whom the John Phillips award was made for 1932–33, and Dr. William B. Castle, of the Harvard Medical School, to whom the award was made for 1933–34, received the medals at the ceremony.

AT the New Orleans meeting of the Electrochemical Society, the Acheson Medal and $1,000 Prize for 1935 was awarded to Frank J. Tone, president of the Carborundum Company at Niagara Falls, for his distinguished work in electrothermics. The presentation will take place on October 10, at Washington, D. C., where the fall convention of the society will be held.

DR. WALTER B. CANNON, George Higginson professor of physiology at the Harvard Medical School, was elected an honorary member of the National Academy of Medicine of Spain at the recent celebration of the two hundredth anniversary of its founding.

PROFESSOR GUÉRIN, director of the anti-tuberculosis vaccination service of the Pasteur Institute of Paris, has been elected a fellow of the French Academy of Medicine.

W. B. HERMS, professor of parasitology at the College of Agriculture of the University of California, has been decorated by the French government with the rank of Chevalier du Mérite Agricole.

AT the annual meeting of the Boston Society of Natural History, held on May 1, the following officers were elected for 1935–1936: *President,* F. W. Hunnewell; *Vice-presidents,* Nathaniel T. Kidder, Glover M. Allen, William M. Wheeler; *Secretary,* Clinton V. MacCoy; *Treasurer,* Augustus P. Loring, Jr.; *Trustees,* Charles H. Blake, Ralph Hornblower, John C. Phillips, Alfred C. Redfield, Charles H. Taylor, William H. Weston, Jr. At the same meeting the annual Walker Prizes in Natural History, offered this year for the best memoir on any subject in the field of general zoology, were awarded to Caryl P. Haskins, Harvard University, for his paper on "The Perception of Sound and Sound Production in Certain Ants" (first prize) and Dr. T. T. Chen, Yale University, for his paper on "Chromosome Studies in Protozoa. I. Observations on Mitosis in Some Opalinids (Ciliata), with Special Reference to the Behavior and Individuality of Chromosomes" (second prize).

AT the meeting of the Louisiana Academy of Sciences held in Alexandria on March 29 and 30, O. L. Meehan, of the U. S. Bureau of Fisheries, Natchitoches, was awarded the gold medal of the academy for his paper on "The Relative Importance of the Plankton Constituents of the Bass Ponds as Measured by their Organic Contents." The twenty-five-dollar prize awarded by the Graduate School of Louisiana State University was given to George H. Lowery, Jr., graduate student in the department of zoology of the university. The subject of his paper was "Preliminary Notes on the Biological Survey of Louisiana—Life Regions and Mammals."

THE Howard Taylor Ricketts Prize of the University of Chicago for 1935 has been awarded to Floyd S. Markham, advanced student in bacteriology, for his paper entitled "Studies in the Submaxillary Gland Virus of the Guinea Pig" and to Sion W. Holley, assistant in pathology, for his paper entitled "Corneal Reactions of Normal and of Tuberculous Guinea Pigs to Tuberculo-protein and Tuberculo-phosphatide." Established in honor of Dr. Howard Taylor Ricketts, who discovered the germ of typhus fever and died from the disease while working in Mexico in 1910, the award is announced each year on May 3, the anniversary of his death. The prize is given to a student or students for the best results in research in either the department of pathology or the department of hygiene and bacteriology.

JAMES H. CRITCHETT, vice-president of the Union Carbide and Carbon Research Laboratories, Inc., New York City, has been elected to succeed Dr. Hiram S. Lukens, of the University of Pennsylvania, as president of The Electrochemical Society, Inc.

DR. ERNEST B. BRADLEY, of Lexington, Ky., was chosen president-elect of the American College of Physicians at the recent Philadelphia meeting. He will take office at the session a year from now. Dr. James Alex. Miller, professor of clinical medicine at the College of Physicians and Surgeons of Columbia University, was inducted as president. He succeeds Dr. Jonathan C. Meakins, professor and head of the department of medicine at McGill University, who gave the presidential address, in which he traced the history of the various colleges of physicians and surgeons. Dr. Arthur R. Elliott, of Chicago; Dr. David P. Barr, of St. Louis, and Dr. Egerton L. Crispin, of Los Angeles, were elected first, second and third vice-presidents, respectively.

AT a recent meeting of the Seismological Society of America, Professor S. D. Townley was elected president of the society. At the same time he relinquished the editorship of the *Bulletin* of the Seismological Society, which he has held since 1911.

PROFESSOR F. ELLIS JOHNSON, head of the department of electrical engineering at the Iowa State College, has been appointed dean of the School of Engineering at the University of Missouri.

PROFESSOR THORNDIKE SAVILLE, since 1932 head of the department of hydraulic and sanitary engineering at New York University, has become associate dean of the College of Engineering.

DR. ANDREW HUNTER, Gardiner professor of physiological chemistry at the University of Glasgow, has been appointed professor of pathologic chemistry at the University of Toronto. He succeeds the late Professor Victor J. Harding.

THE retirement, after serving continuously for forty years, is announced of Professor Ransom A. Moore, head of the department of agronomy at the University of Wisconsin. He will be succeeded by Olaf S. Aamodt, head of the department of field crops of the University of Alberta.

DR. CARL E. GUTHE, director of the Museum of Anthropology at the University of Michigan, has been made chairman of the Division of Social Sciences, established by the regents in May, 1934, at the request of the Social Science Research Council of the university. It will consist for the present of the following departments and schools: anthropology, business administration, economics, geography, history, law, philosophy, political science, psychology and sociology.

DR. HOWARD S. BRODE, professor of biology and curator of the museum at Whitman College, Walla Walla, Wash., for the past thirty-six years, will retire as head of the department at the close of the present

academic year. He will continue to serve as curator of the museum. Dr. Malcolm D. Brode has been appointed acting head of the department for the coming year.

DR. ROBERT T. HATT has resigned as assistant curator in the Department of Mammalogy of the American Museum of Natural History as of July 1, to become director of the Cranbrook Institute of Science.

THE appointment of members of an advisory council for the George S. Cox Medical Research Institute for the study of diabetes, which is one of a group of related medical units affiliated with the University of Pennsylvania, has been announced by Dr. Alfred Stengel, vice-president of the university in charge of medical affairs. Those appointed are: Dr. J. B. Collip, professor of biochemistry at McGill University; Dr. George A. Harrop, associate professor of medicine, the Johns Hopkins University; Dr. Elliott P. Joslin, clinical professor of medicine, Harvard University; Dr. Phillip E. Smith, associate professor of anatomy, School of Medicine, Columbia University; Dr. Rollin T. Woodyatt, professor of medicine, University of Chicago; Dr. O. H. P. Pepper, professor of medicine, University of Pennsylvania.

DR. IRVING LANGMUIR, director of the Research Laboratory of the General Electric Company, gave a lecture at Harvard University on "Films Consisting of One or More Layers of Molecules" on May 7 under the auspices of the Harvard Chapter of the Society of Sigma Xi.

THE annual Phi Beta Kappa address at Colgate University was delivered on April 16 by Professor Douglas Johnson, of Columbia University, who took as his subject "Muddy Thinking."

THE second annual Harry Hayward Charlton Memorial Lecture in Anatomy was delivered on April 30 by Dr. H. B. Latimer, professor of anatomy at the University of Kansas, at the Medical School of the University of Missouri. Professor Latimer spoke on "Growth as Illustrated by Laboratory Animals."

DR. F. H. PIKE, of Columbia University, lectured before the Washington Square College Chapter of the Psi Chi (national honorary society in psychology) on April 17 on "The Nature of Nervous Reactions."

THE annual dinner of the Cornell Medical College was held at the Biltmore, New York City, on May 2. The speakers included President Livingston Farrand, of Cornell University, and Dr. James Ewing, professor of oncology at the college.

FOR several years the department of geology and geography of Northwestern University has had an exchange lecture plan. There have been two exchanges with the University of Cincinnati, one with Washington University, St. Louis, and this year with George Peabody College, Nashville. In this year's exchange, Professor W. H. Haas gave a series of four lectures on "The Geography of the Tropics" on April 11 and 12, and Professor A. E. Parkins filled the return engagement at Evanston on April 25 and 26, speaking on "The Geography of the South."

THE Wilbur Wright Memorial Lecture and conversazione of the Royal Aeronautical Society will take place in the Science Museum, London, by permission of Colonel E. E. B. Mackintosh, director of the museum, on May 30. The lecture will be read by D. W. Douglas, president of the American Institute of Aeronautical Sciences, and designer of Douglas aircraft.

THE sixth annual meeting of the American Association of Physical Anthropologists was held at the Wistar Institute of Anatomy, Philadelphia, on April 25, 26 and 27. The annual public address was delivered by Professor T. Wingate Todd, of Western Reserve University, who spoke on "The Bodily Expression of Human Growth and Welfare." At the annual dinner the guest speaker was Professor C. U. Ariens Kappers, of the University of Amsterdam.

THE ninety-fourth Congress of German scientific men and physicians, which had been arranged for this year, has been postponed to May 24, 1936, when it will be held in Dresden under the presidency of Professor Sauerbruch.

THE dedication of the new library and chemical buildings at the University of Arkansas will be held on June 10. At the exercises for the chemistry building Dean V. L. Jones will preside, and the main address will be given by Dr. Edward Bartow, president-elect of the American Chemical Society.

THE Regents of the University of Michigan have changed the name of the department of chemical engineering to the department of chemical and metallurgical engineering and have authorized the dean and faculty of the College of Engineering to arrange a program in metallurgical engineering leading to the degree of Bachelor of Science in Engineering (Metallurgical Engineering).

THE School of Medicine of George Washington University announces the acceptance of several grants for various research projects as follows: From the Rockefeller Foundation the sum of $25,500 in support of studies in the department of biochemistry; a renewal of the Kane-Kotz Fund of $1,700 for studies on clinical endocrinology in the department of obstetrics and gynecology; from the Eli Lilly Company the sum of $1,200 for a fellowship in biochemistry, and a grant

of $1,800 for the study of the post-pituitary hormones from Parke, Davis and Company.

Two gifts, amounting to $243,000, have been made to the University of Chicago by the Rockefeller Foundation. The larger gift, of $168,000, is to assist in establishing a department of psychiatry and the second gift, of $75,000, is for support of research in the humanities. With the establishment of the department of psychiatry as a division of medicine, there are represented in the south side medical school all the ordinary branches of medical research. The new psychiatric division will maintain twelve beds in a special unit of the University Clinics, and emphasis will be laid on research in the causes and cure of mental disease. Appointment of a psychiatrist as head of the department will be made before July 1, when the new unit will be established.

A GIFT of £10,000 has been received from J. Albert Thompson for the purpose of establishing a commercial laboratory in the University of Edinburgh. This will provide for the immediate requirements in staff and equipment for a laboratory providing the approved methods of training for students for the commerce degree.

A JOINT expedition representing the museum of the University of Pennsylvania and Columbia University has left for Venezuela where, under the leadership of Dr. M. Vincent Petrullo, four months will be spent in investigating the Goajiros, one of the least-known primitive tribes. Accompanying Dr. Petrullo will be Mrs. Gwyneth Browne Harrington, of Boston; Miss Lydia du Pont, of Wilmington, Del., and Lewis Korn, of Philadelphia, assistants in the South American Section of the University Museum, and Dr. and Mrs. Paul Kirchoff, who will represent Columbia University. The group will stop over for a few days at Curaçao and then proceed to Maracaibo, Venezuela, before journeying into the interior. In addition to the University Museum and Columbia University, the expedition is being sponsored also by the Latin-American Institute, Philadelphia, which has recently been formed in response to a demand for an agency to organize and coordinate research and to disseminate information on the entire field of Middle and South American ethnology, archeology, anthropology and linguistics. The forthcoming investigation of the Goajiros is the first opportunity of the institute to carry out actively the purposes of its foundation.

THE British Mount Everest Committee has arranged to send to Mount Everest this summer a small reconnaissance expedition in preparation for a further attempt on the summit in the early summer of 1936, under the leadership of Hugh Ruttledge. The reconnaissance will be led by E. E. Shipton, and will include H. W. Tilman, Dr. Charles Warren, E. H. L. Wigram, L. V. Bryant (from New Zealand), Michael Spender and one or more members from India of the Himalayan Club not yet finally selected.

DR. SVEN HEDIN, accompanied by members of his expedition, arrived in Stockholm on April 15. The London *Times* reports that he was received at the station by a large and cheering crowd, headed by the Duke of Dalecarlia, grandson of the King. Later in the day a deputation from the Swedish Royal Geographical Society waited on him at his home and presented to him a superbly bound publication and the Austrian Minister handed to him a high Austrian distinction.

DISCUSSION

THE ORIGIN OF THE HIGHER FLOWERING PLANTS

WHEN recently reading Dr. A. C. Seward's "Plant Life through the Ages" (1933) the old puzzle concerning the apparently sudden dominance of Angiosperms in Cretaceous time came before me and led to the development of some ideas which may be worth discussing. It has been commonly assumed by botanists than the first Angiosperms were trees or at all events woody plants. This opinion appears to be strongly fortified by the fossil record, the remains found belonging almost exclusively to arborescent forms. But evidence of this sort is probably not conclusive, for such reasons as the following:

(1) In the existing flora, herbaceous plants are especially prevalent in mesophytic and arid situations, and from their mode of growth, as well as the circumstances of their environment, are extremely unlikely to be preserved as recognizable fossils. The deciduous leaves of trees, which often cover the ground after a storm, are very much more likely to be covered up and preserved.

(2) No one can possibly doubt that herbaceous plants abounded during mid-Tertiary time, much as they do to-day. Yet if we examine Knowlton's "Catalogue of the Mesozoic and Cenozoic Plants of North America" (1919) the extreme poverty of herbaceous genera in the recorded extinct floras is astonishing.

Thus the family Ranunculaceae is represented only by a supposed *Thalictrum,* so doubtful that it was not given a specific name. Since then I have found buttercup seeds in the Florinant shales (Miocene), but of the many species of Ranunculaceae which must have been the ancestors of the present flora, there is hardly a trace. The same may be said of numerous other families.

(3) The evolution of the modern flower must have been closely connected with the development of the flower-visiting insects, especially the bees and their relatives. Now the earliest known bees are from the Oligocene (Baltic amber) and the oldest sphecoid wasp is from the Green River Eocene. These Hymenoptera are by no means primitive types, but are essentially similar to those now living. They certainly had Mesozoic ancestors which have not been found.

(4) To this day, bees abound especially in dry regions, such as Arizona, Turkestan, Algeria or the karroo of South Africa. There may be found a prodigious number of species, together with a rich flora to which the bees are adapted. These can, however, be a rather varied flora without a variety of bees, as shown by the Pribilof Islands, which have only one kind of bee (*Bombus kincaidi*).

Thus it appears probable that the developing angiosperms, together with their developing insect visitors, occupied mainly upland and relatively dry regions, and if so, would rarely chance to appear as fossils. The soft, non-deciduous leaves would rarely be scattered on lake shores, and still more rarely would the remains, if preserved, be recognizable. The bees, if present, would be more likely to be recognizable, but they have not been found on the Mesozoic, although they must have existed.

If we assume that the early angiosperms were not woody, but herbaceous, and were at the same time largely confined to uplands or dry regions, it is easy to understand why they do not appear in the fossil record, or rather, are represented by few and more or less doubtful fragments.

I wrote briefly to Dr. Seward on this subject and he at once referred me to the most illuminating discussion in Chapter V of Mrs. Arber's work "The Gramineae" (1934). Here the problem of herbaceous vs. arborescent origins is discussed in the broadest and most lucid way, and it is shown that the assumption that woody types came first is by no means necessarily valid. Zoological evidence is added to the botanical, and the chapter is so full of original ideas that a brief summary is impossible.

Supposing it to be true that a rich herbaceous flora of angiosperms existed during the earlier part of the Mesozoic, how can its existence be demonstrated? Just as rich early fish-faunas have been revealed by the study of scales and otoliths, so it may well be that floras will be proved to have existed by the discovery of small seeds and pollen, and very likely also of calyces or sepals. No adequate search has been made for such objects in the rocks which might contain them. R. P. Wodehouse (1933) has described and illustrated the pollen of the oil shales of the Green River Eocene, and has shown that many species, belonging to thirty-four genera, could be recognized. He states, however, that herbaceous plants, apart from a few aquatics, are not represented. This statement must be qualified by another, that only about a third of the pollen species present has been described and identified. Wodehouse says, "The absence of terrestrial herbs is entirely in keeping with the theory put forward by E. W. Sinnott, that the herbaceous type was developed in temperate regions during Eocene time in response to a progressive refrigeration. At this period terrestrial types were only beginning to be developed." Any one who has considered the slowness of plant evolution, and the small amount of change in the insect fauna since early Tertiary times, can not readily believe that the great and varied herbaceaus flora of to-day had such a recent origin. A really full and adequate discussion of the problem might well require a book, but I venture to suggest that enough has been adduced to justify a minute examination of Jurassic, Triassic and even Permian lake-bed deposits, wherever they are suitably fine-grained, in the hope of finding small seeds or other remains, and perhaps especially pollen, representing an herbaceous flora of angiosperms.

T. D. A. COCKERELL

UNIVERSITY OF COLORADO

THE MOTION OF GLACIERS

AFTER reading Dr. Chamberlin's objection (SCIENCE, December 7, 1934) to my contribution (SCIENCE, November 2, 1934) on "The Motion of Glaciers" he and I had an oral discussion of the topic. This reply (which will have been seen by Dr. Chamberlin before it is sent to the editor) will endeavor to make clear our differences. From it the reader should without further notices be able to come to his own conclusions in regard to the merit of the several contentions. It is of course to be realized that there is not space in SCIENCE for a complete review of the problem. The monograph by Hess which prompted the first notice covers that ground fully if not comprehensively.

From our discussion it developed that Dr. Chamberlin was of the impression that my piece was in some sense unfair. This because I used as tenets of the shear theorists formulations which originated with

him or with T. C. Chamberlin and which he thought should not be so applied. I wish absolutely to disclaim any intention of putting any one in a false light. Further although, in discussion, Dr. Chamberlin said it is his own conviction that inter-grain shift is the chief process of glacier motion there remains a difference of interpretation of the phenomenon that is not cleared up by the apparent agreement in views this adherence suggests. The difficulty is that Dr. Chamberlin, as I understand him, and the shear-motion adherents, insists that glaciers be regarded as essentially rigid masses, whereas the other viewpoint is that the flow function of glaciers derives wholly from a condition of viscosity or plasticity of the glacier ice. "The results speak definitely for the solidity and elastic rigidity of moving glacier ice, and decisively against liquid or viscous flow as the main type of adjustment under stress."[1] It is difficult to see how such a statement can be regarded as any less "sweeping" than the one I made and to which Dr. Chamberlin objected.

The following paragraphs apply in rebuttal to Dr. Chamberlin's point-by-point protest.

(1) "What else besides sliding could cause striation, etc.?" Why, a stiff viscous body holding graving tools large and small. Glass is a viscous substance equally with fluid lava to which latter anti-viscous glacial flow adherents immediately turn for an analogy. Further, even if a glacier is regarded as a rigid crystalline body the fact of pressure-temperature melting equilibrium at its bottom would tend to bring about imbedding in the ice of any rock graving-tool because the projection of such a tool would induce differential pressure with relief by melting and refreezing. Theoretically, one ought to expect no damage to a polished metal surface from application to it of coarse carborundum flakes on the under side of a cake of beeswax.

(2) Intermittent slip. The fact of such slip in the upper cover, marginal and terminal zones of a glacier where the thickness of the ice is insufficient to develop true glacial, that is, viscous flow, is well known and was specifically referred to in the third paragraph of my original notice. But such motion is merely an induced result of the true glacial flow.

(3) Solid shearing of aggregates of granules. Like intermittent slip applies only to marginal and cover ice.

(4) Idiomolecular exchange. Although Dr. Chamberlin here maintains that he has all along upheld the concept that "movement between the granules . . . to be the fundamental mechanism" of glacier motion, such contention does not seem to be in keeping with the declaration made in his author's abstract quoted above. If he visualizes the idiomolecular exchange as a vast succession of dot and dash yieldings between dry crystals the process could perhaps be regarded as non-viscous yield. But it should be realized that the idiomolecular exchange serves primarily (perhaps exclusively) the growth of larger crystals at the expense of smaller ones. Such idiomolecular growth was fully demonstrated to occur under conditions of minimum static pressure and essential freedom from differential stresses by Emden.[2] Further, it is well known to chemists that large crystals immersed in solutions of their constituent material will rapidly consume any small crystals present, but that such idiomolecular transfer proceeds very slowly, if at all, under dry contact. Hence it is again the salt solution, the chief tenet of my concept, that functions ideally to promote the growth of the glacier crystals. However, to return to the main argument, such idiomolecular exchange is not a *modus operandi* of glacier motion. Whether made up of a few large crystals or a vast number of smaller crystals the volume of ice must remain the same. As "explained in various text-books" (one copying from another?) it is faintly implied that the growth of crystals and the melting and refreezing under changing conditions of pressure permits a settling down of the glacier by the closing up (with escape of air) of previously existing "pore" space. Such process may be transiently operative in the upper reaches and upper levels of a glacier but can not be invoked for the main action of glacier motion. On the other hand, without such free space there can be no motion by these means. Where could the melt water go?

Instead, as previously maintained, glaciers wallow down their courses, the crystals growing and shifting in relation to each other through the medium of the liquid salt solution which surrounds them. The total volume of such solution varies with pressure and temperature, its thickness between crystals becoming greater as the size of the crystals increases because of the diminishing surface area. Thus it is constantly being made adequate to its task as a lubricant.

Dr. Chamberlin further protests that I ought not lump the propositions (1) to (4) under the heading shear concept. But as I see it they are all used to bolster up the idea of glaciers as rigid elastic bodies and to provide the conditions which will result in shear as a basic means of glacier motion. Hence the grouping is appropriate.

It is not fitting to ask space in SCIENCE to build up the case for appreciable salt supplies in the precipitation that nourishes glaciers. Further, *ad interim*,

[1] R. T. Chamberlin, Abstract in *Geologisches Zentralblatt*, Vol. 37, 1928, No. 1337, p. 412.

[2] Robert Emden, *Denkschriften d. Schweiz. Naturf. Ges.*, 33, Zurich, 1892.

J. V. Harrison has himself found an explanation for the salt glaciers.[3]

O. D. VON ENGELN

CORNELL UNIVERSITY

A SYSTEM FOR SUBJECT REFERENCE FILES FOR SCIENTIFIC LITERATURE

IN the course of a recent investigation, which has necessitated reading a portion of the extensive literature relating to anaerobic bacteria, the authors have devised a simple system for the routine cross-indexing of topics covered in the scientific articles reviewed. It is believed that the system proposed may be applied with benefit for any field of science, either in listing current publications or as a basis upon which may be built a permanent bibliography for a particular field. The system is intended for personal use, and it will probably be found to work best if applied to a limited field, in which its user is himself working.

In setting up the system it is first necessary to outline carefully the field which is to be covered by the bibliography. A portion of the system which we have found useful in our field may be given in detail to more fully explain the key. For illustration, three general divisions of our present file will serve:

A. Source of isolation
  a. Soil
  b. Dairy products
  c. Food products other than b
  d. Intestinal tract or feces
  e. Body other than d
  f. Water
  g. Miscellaneous

E. Products of metabolism (other than toxin, etc.)
  a. Acids
    1. Acetic
    2. Butyric
    3. Lactic
    4. Propionic
    5. Others
  b. Alcohols
    1. Butyl
    2. Ethyl
    3. Isopropyl
    4. Others
  c. Acetone
  d. Intermediates of fermentation
  f. Gases (CO_2, H_2, H_2S, etc.)
  g. Miscellaneous

G. Serological reactions
  a. Agglutination
  b. Precipitation
  c. Complement fixation
  d. Toxin antitoxin
  e. Miscellaneous

The outline key, part of which is shown in Fig. 1, is printed in skeleton of letters and numerals on the lower half of unlined 4″ by 6″ index cards (we have found it useful to provide for expansion of the outline by extra divisions of each section). The right half of the card is lined for notes.

At the time the original article is reviewed a master card is made, giving complete citation of the author or authors, title of the article and reference. Check marks are then made on letters or numerals, which

[3] J. V. Harrison and N. L. Falcon, *Geological Magazine*, 71: 537, December, 1934.

Welch, W. H.
Morbid conditions caused by *Bacillus aerogenes capsulatus*
Johns Hopkins Hosp. Bul. *11*: 185–204, 1900
A abcdefgh G abcdefgh
E abcdefgh
1234567
1234567

FIG. 1. Showing a convenient arrangement of reference and part of the key.

indicate the topics considered. For each section so indicated on the master card a separate card is to be typed with author and title citation; these cards are marked to indicate only the section in which they are to be filed. Notes or short direct statements of results may be added to the appropriate subject card, if desired. Two files are then maintained, an author file of the master cards and a subject file of the cross index cards.

An advantage of the system is that it eliminates the need for routine briefing of articles. For the average paper, a quick reading or only checking of subject-matter is all that is necessary; it is thus possible to cover several papers or even volumes in one evening. It will be found also that the key is unconsciously memorized and that there is little trouble in checking off topics rapidly and accurately, particularly if the field is limited and is of major interest to the bibliographer himself. The system is a time-saving device, for once the master card is prepared and the correct number of subject cards indicated, ordinary stenographic help can be used to copy the reference to the cross cards. If extended to cover in a systematic fashion all the back volumes of journals containing pertinent papers, it becomes a permanent file from which at least two types of questions may be answered in a minimum of time. These are: (1) what topics are covered by a particular paper (the author of which is known), and (2) what articles deal with any one specialized topic? This latter use is possible only through the multiple filing, and that is possible through elimination of abstracting, always tedious and inadequate.

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ELECTRODES COME IN PAIRS

FOR some years there has been evident on the part of physiologists a tendency to call a pair of electrodes an electrode. Perhaps the tide of this gross misuse of physical language has gained so much momentum that nothing can stop it, but every effort should be made to do so if possible.

The reasons for this misuse of terms are probably that many who use electrodes for physiological or medical purposes are so untrained in physics and chemistry that they do not know the true meaning of the word, and that frequent use of a device which facilitates the application of a pair of electrodes to a nerve or other tissue, as a single unit, leads them to form the habit of thinking of it in the singular.

These reasons do not justify the practice. The word "electrode" has a definite physical meaning and should be used with respect to that meaning. This use of the singular where the plural is meant is analogous to calling a pair of boots a boot, or a pair of gloves a glove. The difference between a pair of eyeglasses and a monocle should serve to stress the point.

The misuse of the singular can not be excused on the ground that in practice one always uses a pair of electrodes, for there are cases in which an electrode may be applied singly, and the singular is needed to designate such a case. Often a diffuse electrode (usually grounded) is applied to one part of the preparation, while a small localizing electrode is applied to the particular structure being studied. This applies both to stimulation and to leading off of electric responses. If the word "electrode" is habitually used to denote a pair of electrodes, no suitable term is left for the single electrode. In short, the use of a word with a definite physical meaning in an improper sense opens the way to endless confusion, and should be heartily condemned.

ALEXANDER FORBES
HARVARD MEDICAL SCHOOL

THE NATIONAL ACADEMY OF SCIENCES.[1] II

Transformations of differential elements: EDWARD KASNER. The simplest type of differential element is a lineal element (x, y, y′) defined as a point with an associated direction. General element transformations, studied by the author, carry a curve, not into a curve, but into a *series* of ∞^1 elements, not in united position. A simple example of a series is a *turbine,* obtained from the elements of a circle by applying a turn T (through a fixed angle) or a slide S (through a fixed distance). All turns and slides generate a 3-parameter group G_3. This is a subgroup of the general group G_{15} which converts turbines into turbines. We next study general isogonal series and equitangential series obtained by applying T and S to general curves. The largest group converting isogonal series into such is shown to be the product of the conformal group and the turn group. The dual theorem gives the product of the equilong group and slide group. Transformation theories are obtained for velocity and natural families in dynamics and also for the dual types. The general transformation of normal congruences has application to optics. Finally, the osculating turbines of general series are studied, giving a wide generalization of the classic theory of evolutes which had its origin in Huyghens's wave theory.

Analysis of 18,000 proper motions derived at the Leander McCormick Observatory: P. VAN DE KAMP and A. N. VYSSOTSKY (introduced by S. A. Mitchell). Proper motions of 18,000 stars between magnitudes 7½ and 14 have been derived photographically in 341 regions. These regions, forming a sample of about one half of one per cent. of the total area of the sky, are representatively distributed north of Declination −30°. The motions have been made absolute by means of the motions of 574 bright stars kindly furnished by the Dudley Observatory in advance of publication. In addition, spectra for 5,200 of these faint stars were secured at the Harvard College Observatory. The more important results are: (1) Corrections to Newcomb's precession constants were found with high precision, due to the large number of faint stars with small motions which constitute an almost ideal "fixed" reference system. (2) The direction to the center of the rotation of the galaxy and the constants of the differential galactic rotation were found practically identical with corresponding figures previously derived by various investigators from the motions of the bright stars. Thus, it is shown that the phenomenon of galactic rotation is not limited to restricted groups of high luminosity stars, but is shared by the general population of the galaxy. The galactic longitude of the center was found to be 321°, in the constellation of Scorpius. (3) The position of the Solar Apex, at right ascension, 19.0 hours and declination, +36°, was found to differ by 15° from the Apex derived with respect to the bright stars. This is thought to indicate a higher percentage of high velocity stars among the apparently faint stars. The results given under (1) (2) and (3) were obtained from one simultaneous solution for the 8 unknowns involved; they are therefore independent of any outside data, except the system of the new "Boss" catalogue. (4) In general, the secular parallaxes are somewhat larger in northern galactic latitudes than in the corresponding southern latitudes, the smallest parallaxes being found not in the Milky Way as might be expected but about 15° away from it, north and south. Furthermore, the parallaxes of the groups of fainter stars in the Milky Way are much larger than had previously been supposed. These all indicate heavy obscuration near the plane of the Milky Way. The results given under (3) and (4) were confirmed in a general way from a discussion of the proper motions in right ascension of some 9,000 faint stars used in parallax determinations at the Allegheny, Johannesburg and McCormick Observatories. (5) A study of the ellipsoidal distribution of motions revealed a clear dependence of the position of the Vertex on absolute magnitude. Thus the stars of large proper motion (predominantly

[1] Continued from page 426.

dwarfs) have their Vertex at galactic longitude 330°, *i.e.*, close to the galactic center. Again, the Vertex found from stars in low galactic latitudes is at 347°, whereas from stars in high galactic latitudes where the proportion of dwarfs is larger, the vertex is found to be at 337°. This same dependence appears clearly in the works of Wilson and Raymond, of Jones and of Hufnagel, although none of them attached great weight to it. Furthermore, a re-analysis of the Radcliffe proper motions of faint stars shows the same effect. Thus, the dwarf stars conform to the simple theory of galactic rotation and only the behavior of the giants, which constitute a relatively unimportant part of the mass of the galaxy, remains to be explained.

Recent advances in our knowledge of the solar chromosphere: DONALD H. MENZEL (introduced by Harlow Shapley). Spectra of the chromosphere, secured by Lick Observatory at the eclipses of 1930 and 1932, have yielded a wealth of new data concerning physical conditions in the solar atmosphere. The spectrograms were calibrated photometrically, and relative intensities of emission lines at various levels have been obtained. Dr. Joseph H. Moore, of Lick Observatory, and I have measured the 1930 moving-plate spectra, and have evaluated the intensity gradients of numerous important lines. Dr. G. G. Cillie, Mr. H. H. Lane and I have investigated the 1932 spectrograms. The following conclusions are based on the best available data from all eclipses, including those of 1905 and 1908. The intensity gradient is logarithmic, *i.e.*, the intensities I at height x cms above the base may be represented in terms of the intensity I_o at the base by the following formula: $I = I_o e^{-ax}$, where a is the decrement constant. a appears to have practically the same value for all lines of a given atom in a particular stage of excitation, irrespective of the magnitude of I_o, which indicates that self-absorption is negligible. Lines of high excitation potential have higher values of a than lines of low excitation potential. For a given element, a is greater for the neutral than for the ionized atom, a result to be expected from ionization theory. a shows a tendency to increase with atomic weight, as if the heavier atoms were "settling out." Of particular significance are the ultimate lines of Ca^+, Sr^+ and Ba^+, for which the respective a values are, within experimental error, directly proportional to the molecular weights. Marked variations in a are shown to exist from eclipse to eclipse and at different points around the sun at a given time. These changes are especially pronounced for the lines of He and He^+. The observed a's for H are from three to five times less than would be expected in an atmosphere of pure H in gravitational equilibrium at 6,000°. The whole character of the chromospheric spectrum bears out an earlier conclusion, *viz.*, that the chromosphere is a hot-spot phenomenon. It appears to be impossible to account for the nature of the spectrum without postulating either (1) the existence of ultra-violet radiation ($\lambda\lambda$ 1,000 – 100) in excess of that to be expected from a black-body at temperature 6,000°, or (2) the presence of high-velocity electrons ejected from the sun.

The shape of the corona and its relation to the sun-spot cycle: S. A. MITCHELL. Measurements of the coronas of 1932 and 1934 have given the surprising result that the 1932 corona 1¼ years before minimum of sun-spots is more elongated than the corona of 1934, which took place almost exactly at the time of spot minimum. A total of 18 coronas beginning in 1893 have been measured and their ellipticities determined on a uniform plan. A close correlation is found to exist between the shape of the corona and sun-spot data. For many years the coronas with long equatorial extensions and pronounced polar rays have been called the "sun-spot minimum type," while the circular corona has been called the "maximum type." All the graphs, no matter what spot or prominence numbers are plotted, without exception tell the same story, namely, that the most pronounced minimum type of corona does not take place exactly at sun-spot minimum nor does the maximum type of corona occur at the time of maximum of spots. As long as 2¼ years before spot minimum the corona is quite as elongated as it is at the time of minimum of spots. The most elongated corona is found 1¼ years before minimum of spots and likewise the corona closest in shape to a circle takes place 1¼ years before spot maximum. The corona of 1934 had lost its pronounced "minimum type" characteristics.

Some rare amphibians and reptiles of the United States: A. H. WRIGHT (introduced by L. Stejneger). Our project for the last ten years has been to photograph and to describe live representatives of every form of amphibian and reptile of the United States. Some 10,000 photos have accumulated. In our search, notes on living representatives of rare, recently described or little known forms have amassed, and we present herewith remarks on the following twenty forms: *Amphibians: Ambystoma cingulatum* Cope; *Ambystoma mabeei* Bishop; *Gyrinophilus porphyriticus duryi* (Weller); *Aneides aeneus* (Cope); *Leptodactylus albilabris* (Günther); *Rana heckscheri* Wright.

Reptiles: Crotaphytus reticulatus Baird; *Sceloporus disparilis* Stejneger; *Sceloporus torquatus cyanogenys* Cope; *Neoseps reynoldsii* Stejneger; *Coluber stejnegerianus* (*conirostris*) (Cope); *Elaphe rosacea* (Cope); *Elaphe subocularis* (Brown); *Lampropeltis alterna* (Brown); *Lampropeltis getulus brooksii* Barbour; *Stilosoma extenuatum* Brown; *Ficimia streckeri* Taylor; *Coniophanes imperialis* (Baird); *Kinosternon bauri palmarum* Stejneger. In addition, some 90 plates of the snakes east of the Rockies are exhibited.

A type in Datura with extra-chromosomal material which in inheritance resembles a recessive: A. F. BLAKESLEE, A. G. AVERY and A. D. BERGNER. A chromosomal type in Datura has been synthesized by replacing a 23-24 chromosome by the modified chromosome. 23-14 and the chromosomal fragment -24, which together compensate for the missing 23-24 chromosome. These chromosomes have been rendered homozygous to form a pure-breeding type $\left(\frac{\cdot 24}{23\cdot 14}\right)_2$ in which the 23-24 chromosome is lack-

ing and in which there is a double dose of extra ·14 material. The plant resembles the secondary 2n + 14·14 type called "Mealy," which also has two doses of extra ·14 material but which does not breed true. The addition of extra ·14 material has relatively little influence upon the appearance of the plant affected. In consequence, plants heterozygous for extra ·14 material can be distinguished from normals only by those skilled in recognition of slight differences among Daturas and then only under exceptionally favorable conditions of growth. The $\left(\frac{\cdot 24}{23 \cdot 14}\right)_2$ type, however, which has two doses of extra ·14 material, is readily recognized. This latter type, if its chromosomal constitution were not known, might be classified as a recessive so far as its breeding behavior is concerned. If crossed either way with a normal, the F_1 appears normal and the type is recovered in the F_2 generation. Most pure-breeding types with extra chromosomal material resemble dominant gene mutants in inheritance. Thus the type $(2n + 2 \cdot \cdot)_2$, in which a ·2 half chromosome is translocated to the 11·12 chromosome, is readily recognized when heterozygous as well as when homozygous for the extra ·2 material. Under certain conditions the dominance appears to be complete, since heterozygous can not be distinguished from the homozygous individuals.

Old and new criteria for determining the relationships of higher plants: WALTER T. SWINGLE. The phylogenetic taxonomy of the higher plants has proved extraordinarily difficult to work out. None of the criteria used for determining phylogenetic relationships are infallible, characters that have high classificatory value in one group may have little or no value in another group. Every new criterion helps to indicate relationships. *Criteria classified into Categories.*—A. Morphological (Categories I. to IV.), B. Physiological (Categories V. to VII.) and C. Genetical (VIII.). Category I. *General Morphology,* covers criterion 1, General Appearance of Plant, Gross Morphology and Color of Organs. Category II. *Special Morphology,* covers criterion 2, Flowers and Fruits; 3. Inflorescences, Branches and Metamers; 4, Leaves; 5, Seeds, Fruits and Pollen Grains; 6, Leaf Traces and Stelar Structures, and 7, Embryo Sac, Endosperm and Young Embryo. Category III. *Anatomy and Cytology,* covers 8, Anatomy of Plant Organs, and 9, Number, Size and Shape of Chromosomes and other Cell Structures. Category IV. *Ontogeny,* covers 10, Germination of Seed, Development of Young Plant, and 11, Teratology, Abnormal Development of Plant Structures. Category V. *Physiology and Chemistry,* covers 12, Serological Diagnosis, and 13, Chemical Composition. Category VI. *Compatibility and Susceptibility,* covers 14, Tissue Compatibilities in Grafts and other Tissue Transplants, and 15, Host-Susceptibility to Parasites. Category VII. *Ecology,* covers 16, General Environmental Relations of Plants; 17, Physical Life History Requirements and Limitations; 18, Nutritional Requirements and Limitations, and 19, Special Adaptations of Plants to Environment. Category VIII. *Genetics,* covers 20, Cross Breeding Capabilities; 21, Cytogenetic Characters of Chromosomes and Grouping into Genoms; 22, Cytonomic States of Nuclear Association; 23, Effects of Pollen Parent on Endosperm (Xenia); and 24, Effects of Pollen Parent on Maternal Tissues (Metaxenia). Some of these criteria have been in use since the dawn of human history; several have come into use during the present century and two or three have been discovered during the last decade. Studies of the higher plants made by using many or all of these criteria will undoubtedly lead to a much clearer picture than we now have as to their phylogeny. Such studies will also be of great help in making use of remote relatives of our cultivated plants in creating hybrids of the newly discovered allopolyploid type. These hybrids in many cases show superior vigor, hardiness and disease resistance.

A simple factor affecting the velocity of ionic oxidation-reduction reactions in aqueous solutions: Equivalence of valence change: PHILIP A. SHAFFER. Among the most elusive and obscure problems of chemistry are those concerning the specific velocity of reactions. Modern theory of the subject, although elaborate, is inadequate. Based chiefly on the concept of activation energy, it deals only with physical quantities which so far do not describe fully the characteristic *chemical* properties of atoms and molecules on which both reactions and their rates presumably depend. It is therefore not surprising that present theory is able to predict the velocity of a given reaction only by interpolation from values determined by experiment; without experiment no prediction is possible. Although to this extent apparently successful with many reactions, it seems doubtful whether the concept of activation energy alone, however elaborately treated, can account for the wide differences in velocity found among a large group of supposedly similar reactions of a simpler sort, namely, ionic oxidation-reduction reactions in aqueous solution. While many ionic reactions are immeasurably rapid and are therefore said to require only little energy for activation, others involving the same reactants with different partners may be immeasurably slow. There are many cases where ionic substances, both demonstrably "active," refuse to react (or react slowly) in the same solution, but react rapidly when separated in the form of an electrolytic cell or in the presence of suitable catalysts. To account for this rather surprising behavior some other ideas beside that of activation energy appear to be necessary. In seeking for an explanation, a number of oxidation-reduction reactions between both inorganic and some organic substances have been roughly surveyed as to their relative rates. It appears to be a somewhat general, although not universal rule, that where the "permissible" (and dynamically possible) valence-change of oxidant and reductant is equal—bimolecular reaction being therefore possible—the reaction is relatively rapid, while for cases in which the valence-changes are unequal the reaction rates are much slower, apparently for the reason that reaction must await for the proper three-body collision—a much less frequent event. Support for this simple interpretation is afforded by the fact that certain catalysts

of the latter class of reactions are capable of mediating the oddness of valence-change by undergoing both, thus permitting reaction to occur by a sequence of bimolecular steps. In several cases observed catalytic activity of the substance has led to the discovery of an additional valence state not previously suspected. It seems probable that mediation of an odd valence-change is a common mechanism for the action of catalysts in oxidation-reduction reactions. It is thought that this idea may account for the necessity for certain catalysts in biological oxidations; it appears to give new significance to the property of "two-step" oxidation-reduction possessed by various respiratory pigments, the theoretical analysis of which has been given by Michaelis.

Solutions of the wave equation in spheroidal coordinates: J. A. STRATTON (introduced by John C. Slater). It has been shown that the Schrödinger equation, including the wave equation as a special case, is separable in eleven systems of coordinates only. Of these eleven systems, three alone have been investigated with a thoroughness sufficient to meet all the demands of physical problems. Of those remaining, three more are of outstanding practical importance. The functions of the elliptic cylinder, the prolate spheroid and the oblate spheroid include as special cases the functions of the sphere and the circular cylinder, and are adapted to problems involving slits and flat strips, circular disks and rods of finite length. It is the object of the present investigation to establish the properties of these functions in a detail approaching that known for the Bessel and Legendre functions. On separation of the wave equation in any of the three coordinate systems named it appears that both angular and radial functions satisfy a differential equation of the type $(1-z^2)\,w'' - 2(a+1)\,zw' + (b-c^2z^2)\,w = 0$, wherein the separation constant b is restricted to characteristic values such that one particular solution exists which is finite at the regular points $z = \pm 1$. Asymptotic solutions appropriate to the region of large values of z are defined and normalized in the manner most convenient for physical problems. In diffraction problems, an expansion of a plane wave in terms of the functions of the elliptic cylinder or spheroid is required, and this, as well as the nature of the usual boundary conditions, necessitates a knowledge of the behavior of the functions of both the first and second kind in the neighborhood of $z = 0$. The analytic continuation of both asymptotic solutions into the origin is attained by means of contour integrals and thus expansions of the two independent solutions appropriate to all regions of the z-plane are available, together with their analytic connections.

Arc spectra of hydrogen and deuterium: R. W. WOOD and G. H. DIEKE. It was shown many years ago by Kiuti that in the secondary or molecular spectrum of a hydrogen arc between tungsten electrodes, many of the strong lines obtained with the hydrogen vacuum tube were missing, and others were relatively strong. The matter has now been more fully investigated with higher dispersion, and is discussed from the theoretical standpoint.

Remarks on the measurement of the magnetic moment of the proton: OTTO STERN (by invitation). *Spectroscopic method.* By measuring the frequency change of a spectral line in the magnetic field, the energy change of the atom $\Delta E = \mu H = h\Delta\nu$ is determined. In fact, only the difference in the energy changes of two states of the atom can be measured in this way. At least one of the two states must be an excited one. The *molecular ray method,* on the contrary, allows the measurement of the magnetic moment of a single state, the normal state of the atom. This is valuable not only for the treatment of some fundamental problems (space quantization, etc.), but also for the problem of measuring very small moments. Therefore, it is possible to measure the magnetic moment of the proton ($\mu \approx 10^{-23}$ e.s.u.), a problem not yet solved by the spectroscopic method. The reason for this is a fundamental one, the uncertainty principle of the wave mechanics.[1] This principle stipulates that the uncertainty δE of the measurement of the energy is connected with the length of time of the measurement δt by the relation $\delta E \cdot \delta t \approx h$. Since the lifetime of an excited state of the H-atom is less than 10^{-8} seconds, the uncertainty of the energy measurement is $\delta E \approx \frac{6.10^{-27}}{10^{-8}} = 6.10^{-19}$ erg. The energy change in the magnetic field, due to the magnetic moment of the proton, is $\Delta E = \mu H \approx 10^{-23}.\ 6.10^{4} = 6.10^{-19}$ erg in a field of 6.10^4 gauss. This means that even in such a strong field $\Delta E \approx \delta E$, or the uncertainty in the measurement is as large as the quantity to be measured. Under the conditions of the molecular ray method, δt corresponds to the time during which the atom is in the magnetic field, at least 10^{-4} seconds under the usual conditions. This means that in this case the uncertainty of the measurement δE is only 10^{-4} of the quantity itself ΔE. The actual measurements, carried out first in the Hamburg Institute of Physical Chemistry, fell very much short of this limit of error. Nevertheless, the measurements gave a very interesting result, about $2\frac{1}{2}$ nuclear magnetons. Dirac's theory, very well confirmed in the case of the electron, predicts a value of 1 nuclear magneton for the proton.

SCIENTIFIC APPARATUS AND LABORATORY METHODS

SIMPLIFIED EQUIPMENT OF SMOKING KYMOGRAPH DRUMS

WHERE no separate room can be set aside for smoking kymograph drums, both the experimenter and the instructor is confronted with the necessity of smearing the paint and equipment of the laboratory as well as the clothing of the students with the excess soot. The former difficulty is also one of the frequent and serious

[1] For the spectroscopic problem, *cf.* W. V. Houston and Y. M. Hsieh, *Phys. Rev.*, 45: 263, 1934.

objections raised to psychological work in state hospitals and institutions. The equipment described in this note obviates both of these objections and its simple construction commends it for general use. A line drawing of it is shown in Fig. 1.

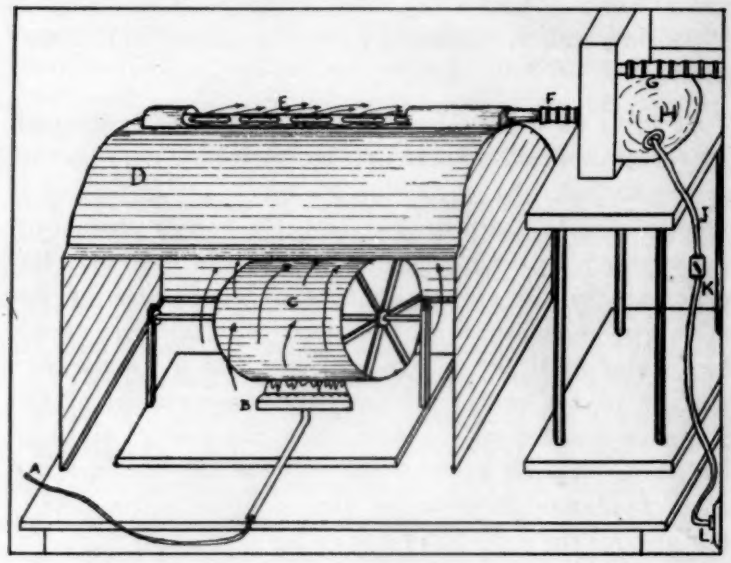

FIG. 1. Drawing of a simple form of equipment for smoking kymograph drums.

The essential parts of the equipment consist of a hood (D) made of about 22 gauge tin and a vacuum sweeper motor (H) mounted on a stand of suitable height and equipped with extension hose (G) which is furnished with the motor. The hood is made large enough to accommodate the drum on its stand (C). In the top are draught vent holes (E) through which the excess soot is drawn away from the flame (B). The soot from the hood is drawn into the intake of the motor by means of a short length of extension hose (F). It is then passed on through to the outlet of the motor and into the hose (G) which replaces the usual bag. The extension hose (G) can then be placed out through a convenient window and all excess soot is taken outside. No changes are made in the electrical connections of the motor (J), except to place a line switch (K) at a convenient place. The height of the motor stand and of the top of the hood depends on the height of the drum stand to be used.

The motor is obtained at any store dealing in used vacuum sweepers. They will also furnish, usually without extra charge, any reasonable length of hose. If carefully and competently chosen, the motor can be expected to give unlimited service after reconditioning. The model outlined here has been in use over three years without any expense for upkeep.

The entire equipment can be secured locally at a cost of about $10.00 to $12.00. The only objection that has appeared in three years of use is the noise made by the motor. This is, of course, similar to that made by a sweeper in ordinary household use. This particular model also works better when the gas is not previously passed through benzine, as is done in some laboratories. It has proved completely satisfactory as far as its main purpose is concerned, and is readily portable either from one room to another or from the laboratory to an outside institution.

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A PARAFFIN BLOCK COOLER FOR USE WITH THE MICROTOME[1]

IN the preparation of serial sections, it is desired to obtain paraffin ribbons that show little or no compression. Such a result with small blocks of tissue facilitates the enumeration of sections when this is a necessary prerequisite to mounting, and also greatly decreases the time entailed in spreading. When only a few sections are necessary, cooling the block on ice previous to cutting is the usual procedure, but for superior results in a long series a continuous supply of cold air is desired.

Foot and Strobell[2] in sectioning eggs of Allobophora devised an apparatus quite comparable to an air-conditioned room. The microtome is placed on a rubber sheet. The cooler, a double-chambered copper box, is superimposed, thus utilizing the rubber sheet as the bottom of the compartment. By means of a glass top that forms the upper surface of the inner chamber and arm holes in the side, one can operate the microtome *in situ* with full view of his movements. A freezing mixture of ice and salt placed between the two compartments allows for a reduction of temperature to twenty-five degrees Fahrenheit.

Grave and Glaser[3] utilized an apparatus that "is essentially a hollow truncated pyramid, open at both ends, and suspended in an inverted position from a standard, so adjusted that the lower end of the chute is at a convenient distance above the knife. At the upper end of the inverted pyramid, and surrounded by it, is a tray whose dimensions are less than those of the base of the chute. This tray is filled with crushed ice, and from one corner of it a drain leads the water to the escape from the lower end of the air channel."

With the idea of utilizing the principle of Grave and Glaser[2] but controlling the cold air supply, a cooler has been devised. The cooling chamber consists of a tin receptacle six inches in diameter and eight and one fourth inches in height. Copper tubing, one fourth inch in diameter, coiled within from the air inlet A, to the outlet B, serves as a medium for the passage of

[1] From the Department of Anatomy, The University of Rochester, School of Medicine and Dentistry, Rochester, N. Y.

[2] K. Foot and C. Strobell, *Biol. Bull.*, 9: 281–286, 1905.

[3] C. Grave and O. C. Glaser, *Biol. Bull.*, 19: 240–242, 1910.

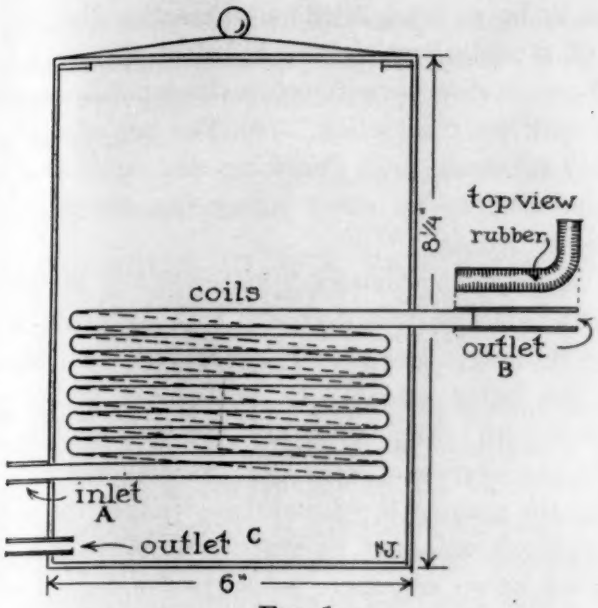

FIG. 1

air throughout the cooling chamber. The inner surface of the latter is painted with black asphaltum, while the exterior is overlaid with asbestos. The protruding portion of the copper tubing forming the inlet A is connected by rubber tubing to a calcium chloride tube for the purpose of dehydration, and in turn to a compressed air supply. The copper tubing, insulated with rubber, passing through the wall of the cooling chamber to form the outlet B, is bent at right angles horizontally, thus permitting direction of air at the surface of the block, and yet allowing for placing the cooler to one side and in front of the microtome. The height of the tubing at this point is variable with the type of microtome employed, but should be so placed as to allow full utilization of the cold air supply. If desirable, an outlet C, as a drain, may be inserted.

In actual operation, the ice chamber is filled with cracked ice, ice and salt or other freezing mixtures. For purposes of this laboratory, the former gave a temperature range sufficiently cool for cutting during the summer months. The microtome and cooler are so oriented that the cold air emitted at the outlet B will play directly on the cutting surface of the preparation. The distance of the former from the latter may be determined by the extent of cooling desired. A few sections are cut without turning on the compressed air. Having thus obtained the basis for a ribbon, a gentle stream of air is directed at the block and cutting is resumed.

Contrary to an opinion that may occur to the reader, the air draft created does not hinder manipulation. During a period of seven months' use, no difficulties were experienced with electrification of the paraffin ribbon.

GERMAIN CROSSMON

SPECIAL ARTICLES

CRYSTALLINE CARBOXYPOLYPEPTIDASE

CARBOXYPOLYPEPTIDASE splits the amide linkages of certain amino-acid compounds, such as chloracetyl tyrosine, tyrosyl tyrosine and leucyl glycyl tyrosine, with the liberation in each case of an amino-acid which in the intact compound has a free carboxyl group.[1] I have isolated from bovine pancreas a crystalline water-insoluble protein which attacks chloracetyl tyrosine. Peptic digests are also attacked, even in the presence of formaldehyde. Other substrates have not been tested, so it is not yet certain that all the supposed substrates of carboxypolypeptidase are digested by a single enzyme. It may be that what has hitherto been called carboxypolypeptidase is a group of different enzymes.

Recrystallization of the globulin does not change its carboxypolypeptidase activity but frees it of proteinase. Heating a solution of the crystalline globulin until half the protein is coagulated results in destruction of half the solution's activity. These facts are strong but not conclusive evidence that the crystalline protein is identical with the enzyme whose activity has been measured. A solution of the crystalline globulin diluted to attack chloracetyl tyrosine at the same rate as a given crude extract of pancreas likewise attacks a formolized peptic digest at the same rate as the crude extract. This fact is strong evidence that the enzyme in the crude extract which attacks chloracetyl tyrosine is likewise responsible for the digestion of the formolized peptic digest. Finally, the fact that the crystalline globulin digests a peptic digest even in the presence of formaldehyde proves that the presence of the free amino groups of neither enzyme nor substrate is essential for carboxypolypeptidase activity. No proteolytic enzyme of the pancreas other than carboxypolypeptidase is known to be active in the presence of formaldehyde.

In outline the preparation of the crystals is as follows. To the spontaneously activated turbid fluid which exudes when frozen pancreas is allowed to stand overnight at 5° C., 5 N acetic acid is added until the solution is green to brom cresol green. The acid solution is kept at 37° C. for two hours and the clotted suspended matter is filtered off. The filtrate is diluted with ten times its volume of water. The resulting precipitate is allowed to settle, the supernatant solution is rejected and the suspension is filtered. Water is added to the precipitate to give a suspension twice as active as the original turbid fluid and then 0.2 M

[1] E. Waldschmidt-Leitz, *Physiol. Rev.*, 11: 358, 1931; M. Bergmann, SCIENCE, 79: 439, 1934.

$Ba(OH)_2$ is added until the suspension is pink to phenolphthalein. Whereas NaOH would dissolve all the dilution precipitate under these conditions, $Ba(OH)_2$ dissolves only a part of the protein but all the carboxypolypeptidase. After removal by centrifugation of the undissolved protein 1 N acetic acid is added to the supernatant solution until the solution is orange to phenol red. The globulin crystals thereupon appear, promptly if the solution is seeded, slowly if it is not. The protein can be dissolved with NaOH and recrystallized by neutralization.

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THE EFFECTS OF PITUITARY IMPLANTS AND EXTRACTS ON THE GENITAL SYSTEM OF THE LIZARD

OVULATION has been induced in a serpent, *Xenodon merrimi,* six days after five homoplastic whole pituitaries were implanted.[1] Hypertrophy of the genital system has been produced in Lacerta[2] and in young alligators[3] by means of mammalian pituitary extracts. Removal of the pituitary causes atresia in the testes of the garter snake (*Thamnophilis sirtalis* and *Thamnophilis radix*) followed by a partial restoration to normal when pituitaries are implanted.[4]

In a series of experiments carried on between October 30, 1933, and April 4, 1934, fifty-five females and seventy-nine males of *Anolis carolinensis* received injections of Antuitrin S (human pregnancy urine extract, Parke Davis), while twenty-five females and twenty-five males received injections of sheep pituitary (whole pituitary extract of Parke Davis). Approximately fifty animals were kept as controls. A single dose with either extract was not more than .02 cc diluted with two or three volumes of cold-blooded Ringer. This proved to be the maximum dose that was safe to use.

The males responded very completely to both extracts and could be very easily distinguished from controls in the following particulars: (1) The dorsal crest along the neck and back was raised, often to the height of an eighth of an inch. (2) The hemipenes could be everted. (3) The testes were often enlarged to two or three times the size of those of controls. (4) The epididymis and vas deferens were always greatly enlarged. In an extreme case of hypertrophy a single loop of the epididymis was found to be at least fourteen times the diameter of that of a control which was killed at the same time. (5) Spermatozoa were found in the epididymis after the fourth daily injection. (6) The vas eferens was slightly enlarged. (7) Courtship and fighting were a common activity on every sunny day during winter and early spring.

In regard to the females, hypertrophy of the ovaries and oviducts was produced with both Antuitrin S and sheep pituitary, but actual egg-laying resulted only with the latter extract. Some females which were injected with sheep pituitary retained mature ova within the ovaries. These eggs were slowly resorbed during the ensuing three months. Neither the ovaries nor oviducts enlarged as much with Antuitrin S as with the sheep extract. After twelve injections of sheep pituitary, two eggs were laid on March 23 and three more the next day. A sixth egg was laid on April 11. The first egg to be laid by any of the controls was on April 18 and a second egg on May 8. No more eggs were laid by controls until June, July and August.

The metabolism of injected animals was greater than that of controls as was shown by an increase both in appetite and in the amount of food eaten. Also, food was required oftener. General activity and speed of movement were undoubtedly greater. Moulting occurred more frequently.

The after-effects were noticeable. Four months after the last injection found many of the treated lizards to be persistently thin, although on an average they ate more than the controls. A few died apparently of starvation while controls, which had received approximately the same amount of food, lived in a perfectly healthy condition.

Complete details concerning these experiments with mammalian pituitary extracts are to be reported later.

It may be of further interest to state that in connection with some experiments with pituitary implantations in *Anolis carolinensis* (December 21, 1933, to April 14, 1934), one female which received four whole pituitaries (taken from males of the same species) failed to ovulate, but the genital system was approximately twice the size of that of controls. Out of three females which received five similar homoplastic implants, two died before ovulation (autopsy showed hypertrophy of the genital system) but the remaining female actually laid two eggs, one on March 24 and another four days later. Another female, which received three frog pituitaries, laid an egg on April 12.

When the two females last mentioned were killed (April 14) they showed mature ova just ready to leave the ovary. Only one ovary in each female, however, contained a mature ovum, although the oviducts

[1] B. A. Houssay, *Compt. Rend. Soc. Biol.*, 106: 377–378, 1931.
[2] M. Herlant, *Arch. de Biol.*, 44: 347, 1933.
[3] T. R. Forbes, *Proc. Soc. Exp. Biol. Med.*, 31: 1129, 1934.
[4] W. H. Schaefer, *Proc. Soc. Exp. Biol. Med.*, 30: 1363–1365, 1933.

on both sides were equal in size and enlarged to the maximum condition necessary for ovulation. None of the twenty-five controls ovulated during the period of this investigation.

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DISCREPANCIES IN THE VALUE OF THE AEROBIC REDUCING INTENSITY OF THE YEAST CELL AND STARFISH EGG[1]

THE recent appearance of a paper by Green[2] on the oxidation-reduction potentials of cytochrome c has brought to light a discrepancy in the value of the aerobic reducing intensity of Fleischmann's yeast cells, as estimated (a) from the reactions of penetrating oxidation-reduction indicators of the Clark series, (b) from the reaction of the naturally occurring oxidation-reduction system cytochrome c.

As Keilin[3] has reported, well-aerated yeast shows none of the bands of reduced cytochrome. For cytochrome c Green[4] has reported E_0' values of about +0.125 v. for pH values between 4.59 and 7.14. Since in aerated yeast all components of cytochrome are so far oxidized that the bands of the reduced form are not detectable, we may safely assume that cytochrome c is at least 90 per cent. oxidized. Assuming for the yeast cell a pH value between 6.0 and 7.0 and taking for cytochrome c within the yeast cell an E_0' value of +0.125 v., we get for the aerobic reducing intensity of aerated yeast cells a value equal to or greater than +0.18 v. (0.125 plus 0.058 $\log_{10}$ 9).

The stated value of the aerobic reducing intensity of these cells, as estimated from the reaction of penetrating oxidation reduction indicators, will depend upon the intracellular pH which we assign to the yeast cell. For a very large number of diverse cells Chambers and his collaborators[5] have found a cytoplasmic pH of 6.8 ± 0.2. Fleischmann's yeast cells stained with methyl red or propyl red take on the alkaline coloration of these dyes, indicating a pH value equal to or greater than 5.8 with methyl red, and equal to or greater than 6.2 with propyl red. It would certainly seem safe, therefore, to assume for the cytoplasm of the yeast cell a pH equal to or greater than 6.0.

In Table I is shown in tabular form the values of the aerobic reducing intensity of Fleischmann's yeast cells, as estimated from the reaction of penetrating oxidation-reduction indicators (previously reported by Beck and Robin[6]) if we assign to the cytoplasm of the yeast cell pH values of 6.0 and 7.0, respectively.

TABLE I

Indicator	E_0' values pH 6.0	pH 7.0	Condition in aerated yeast cells	Estimated value for aerobic reducing intensity pH 6.0	pH 7.0
Toluylene blue	0.162	0.115	Reduced	0.124 or less(1)	0.077 or less(1)
Thionin	0.092	0.062	Partially reduced at	0.092(2)	at 0.062(2)
Cresyl blue	0.089	0.047	Largely oxidized	0.103 or more(3)	0.061 or more(3)
Methylene blue	0.047	0.011	Largely oxidized	0.061 or more(3)	0.025 or more(3)

(1) Potential values estimated on assumption that toluylene blue is at least 95 per cent. reduced.
(2) Potential values estimated on assumption that thionin is 50 per cent. reduced.
(3) Potential values estimated on assumption that cresyl blue and methylene blue are at least 75 per cent. oxidized.

It is quite evident that whether we assume a cytoplasmic pH value of 6.0 or the much more probable value of 7.0 the aerobic reducing intensity of the yeast cell, as estimated with the penetrating indicators, is decidedly more negative than the value which we estimate from the reaction of cytochrome.

Chambers, Pollack and Cohen[7] had noted a similar though smaller discrepancy in their microinjection experiments on starfish and sand-dollar eggs. K_4 indigo tetrasulphonate, E_0' value at pH 7.0 of −0.047 v., is not perceptibly reduced aerobically; ethyl Capri blue, E_0' value at pH 7.0 of −0.072 v., is definitely partially reduced. This discrepancy is in all probability due to the fact that sulfonated dyes are reduced by the cellular dehydrogenase systems much more slowly than are basic dyes (as ethyl Capri blue) having E_0' values of the same order.

It is felt that these discrepancies should be stressed, since they indicate that at least under aerobic conditions the underlying kinetic factors which determine whether a given oxidation-reduction indicator, or other reversible oxidation-reduction system, shall be present within a living cell chiefly in the oxidized or the reduced state, are affected not only by the oxidation-reduction potential of the indicator (or system) but also by its chemical nature. The failure of most workers to note similar discrepancies is probably due to the fact that most of the indicators having oxida-

[1] From the Lilly Research Laboratories, Marine Biological Laboratory, Woods Hole, Mass.
[2] D. E. Green, *Proc. Roy. Soc. B*, 114: 423, 1934.
[3] D. Keilin, "Ergebnisse der Enzymforschung," II. S. 239. Leipzig, Germany. Akademische Verlagsgesellschaft, 1933.
[4] *Loc. cit.*
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tion reduction potentials in the neighborhood of the ones showing partial reduction are closely related chemically to the latter.

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ROLLER CANARY SONG PRODUCED WITHOUT LEARNING FROM EXTERNAL SOURCES

SINCE May 31, 1934, twelve roller canaries have been born and reared in soundproof cages, without hearing a song from any non-isolated bird. Eight are males, and four are females. Daily recordings of their vocal responses have been made on aluminum disks, motion picture films, or strobophotographic records.

The contest roller canary song consists of vocal effects known as rolls and tours. They are distinguished on the basis of sonance, or successive auditory fusion. In a roll, the successive pitch changes are perceived as unitary, whereas in the tour the patterns are perceived as discrete units. Physically, the distinction is one of rate of the successive patterns, the rolls having sufficient rapidity to be fused in auditory perception.

The basic song consists of a sequence of hollow roll, hollow bell, schockel, flutes and water roll. The first four, when graphed for rate of successive patterns, show a decreasing rate resembling a typical muscular fatigue curve. The rolls and tours of the main sequence are added to, substituted for or embellished by individual birds. The added effects are the bass roll, glucke, glucke roll, water glucke, schockel, deep bubbling water tour, bell roll, bell tour and bell glucke. It is rare that a single bird has all the effects in his song. The number generally varies from five to ten.

By January 7, 1935, the date of this writing, all the isolated males had produced recognizable effects of the roller canary song. These data have been checked by Mr. Frank H. Bires, of Whittier, California, an outstanding contest judge.

Nest 1. Males 51, 52 and 53, each aged 212 days, produced a hollow roll, schockel, flutes and water roll. Males 52 and 53 produced a hollow bell, and Male 51 a bass roll, bell roll and bell tour.

Nest 2. Male 24, aged 210 days, produced a schockel, flutes, water roll, hollow roll, deep bubbling water tour and water glucke.

Nest 3. Males 56, 57 and 58, each aged 163 days, sang a water roll and flutes. Males 56 and 57 developed a glucke and bell roll. Males 56 and 58 produced a hollow roll and schockel. Males 57 and 58 produced a water glucke, Male 56 the only bell tour in the nest, and Male 58 the only water glucke which has yet appeared in Nest 3.

Nest 4. Male 60, aged 224 days, produced a glucke, flutes, bass roll, hollow roll, hollow bell and bell glucke.

Taken together, the isolated birds produced all the effects. Four of them, Males 51, 52, 53 and 60, had from four to six effects when breaking into the mature roller song for the first time. They were subjected to inhibiting factors incidental to the original experiment, possibly the excessive heat, or, perhaps in the case of the first three, the fighting which often occurs when males are in the same cage. The other four, Males 24, 56, 57 and 58, developed one roll and tour after another from their baby song. The latter three were isolated at the first appearance of baby song, before any roll or tour appeared. The baby song is for the most part a nonsense melody of choppy notes covering a wide pitch range. The earliest baby song appeared at 60 days and the latest at 149 days.

Rolls appeared earliest in the cases of Males 51 and 52, specifically, at the age of 110 days. The slowest to develop a roll was from the same nest, Male 53, who was 179 days old at the time.

Males 24, 51, 56, 57, 58 and 60 heard no rolls or tours of any kind prior to producing them. Males 51 and 52 heard each other. Male 53 heard the song of Male 24.

The females have produced only a characteristic chirp and simple series of call notes. According to professional canary breeders, the female rarely has any of the rolls and tours. With this assurance, the mothers in this study were left with their young until weaned, the period varying from 25 to 40 days. The notes of the canary hens were observed and recorded, and no semblance of rolls or tours appeared. The males used in breeding were removed from the soundproof cages before the female was placed with the eggs. The eggs were removed from the breeding cage daily until all had been laid.

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